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GUIDEPOST FOR TECHNICAL READERS
MAY, 1949, *PROCEEDINGS*

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AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

PAPERS

AN ENGINEERING CONCEPT OF FLOW IN PIPES

BY CHARLES W. HARRIS,[1] M. ASCE

SYNOPSIS

Today enough is known about flow in pipes, both fundamentally and experimentally, to form a secure basis for consolidation. Holding the physical contacts and keeping the rational elements open to inspection are the secrets of safe advancement.

The relations of smooth and rough pipes, and of large and small pipes, are here expressed by correlated equations rather than by separate empirical formulas. A specific roughness coefficient, by which a surface may be designated and which remains fixed for extreme ranges of diameter replaces a variable coefficient. An index of pure turbulence indicates the velocity at which the friction factor becomes constant. A formula for smooth pipes is recommended also for rough pipes under certain circumstances. Correlation is shown between artificially prepared random roughness and a variety of commercial pipes. A method is furnished for distinguishing surface losses from localized losses, thus exposing the latter for corrective measures. All the equations are related through simple algebraic expressions.

INTRODUCTION

The following facts and principles are arranged to promote a correct method and, in so far as possible, a rational procedure, for computing flow in pipe lines. The inclusion of oils and other fluids need not defeat this purpose; nor does it imply that, to select a friction factor, the viscosity of water must always be known. However, a few old traditions must be firmly set aside.

A reader who is overconscious of the meagerness of his preparation in hydraulics should not despair. It is the nature, not the extent, of the groundwork that counts. He should see in the Chezy formula, $V = C\sqrt{R\,s}$, the embodiment of simple physical meaning whether the equation is solved for

NOTE.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by **October 1, 1949.**

[1] Prof., Hydr. Eng., Univ. of Washington, Seattle, Wash.

velocity, as commonly found for open channels, or for lost head as H. Darcy[2] and Julius Weisbach[3] preferred for pipes. He should know that a constant coefficient in either case confirms the Newton principle of similarity which demands the dissipation of energy in proportion to the kinetic energy involved. If he chances to be a mature engineer, he may find the article mainly corroborative of his early training, although at times it may be sharply contradictory to it. When contrary to his training, it may still agree with his experience.

One who is well grounded in fluid mechanics should not presume that the writer is renouncing that subject when at times his interpretations are not conventional. Absence of logarithmic formulas for resistance factors does not imply that empirical links render them too inaccurate, but that a complicated formula is unnecessary.

The agreement of friction factors for partly turbulent flow in a smooth and in a rough pipe should be emphasized, but not to the extent of obscuring the equally important fact that extreme contrast exists between the two at those higher velocities which produce complete turbulence in a rough pipe. This condition applies to all fluids. No one should feel secure in the knowledge of his limited field since this overlapping is so universal.

An attempt is made herein to express the physical meaning in a simple manner, using only the mathematics with which the average reader is familiar.

General Principles

The flow of fluids through pipes has long been computed by a closely organized combination of two distinct types of formulas—one rational and the other empirical. An efficient use of such a combined method requires keen discrimination as to the nature of these component parts. To make full use of the rational aspects, one should observe that a fluid follows one or another of three distinct modes of physical behavior depending on the circumstances. These three modes are revealed successively when the flow through a moderately rough pipe is changed from zero to some high velocity.

Roughness does not appreciably change the resistance in nonturbulent flow because in this parabolic distribution there is no velocity at the surface of contact. However, when the velocity is increased until the viscous drag along the wall becomes greater than the internal stability, the slightest cross component of velocity permits the streamlines to buckle. The resulting turbulence is projected throughout the cross section of the pipe with the exception of a thin boundary layer that still resists the penetration of cross components. This mixed type of flow constitutes partial turbulence, the boundary layer still being nonturbulent. Roughness here, also, does not materially change the resistance, since the roughness elements are shielded by the boundary layer.

A velocity is eventually reached at which the boundary layer has become thinned to a negligible film and the shape of any roughness becomes exposed

[2] "Recherches expérimentales relatives au mouvement d'eau dans les tuyaux," by H. Darcy, Mallet-Bachelier, Paris, 1857.

[3] "Principles of the Mechanics of Machinery and Engineering," by Julius Weisbach, 1st American Ed., edited by Walter R. Johnson, Lea and Blanchard, Philadelphia, Pa., 1848, Vol. 1, p. 391.

to the direct action of the moving fluid—the rougher the surface the lower the necessary velocity to establish this condition. The pattern of flow, once established by the topography of the solid boundary, remains unchanged for all higher velocities and the friction factor remains essentially constant. There may be a minor secondary influence of viscosity tending to render the pipe wall smoother; but with roughness elements varying widely in size and shape this influence is negligible at any one velocity.

Most engineers who are familiar with the use of the Reynolds number do not care to use it simply to show familiarity. They should however recognize in it the extent to which large physical dimensions dwarf the influence of viscosity, and should review the following facts: First, viscosity plays no part in the loss of head in the fully turbulent flow most frequently encountered in moderately rough pipes and channels. Second, viscosity is the direct source of loss in nonturbulent flow such as found in heavily viscous oils or even in water in small pipes and some laboratory models. Third, for small changes in viscosity in partly turbulent flow, a given percentage of change in viscosity causes approximately one fourth of that percentage of change in lost head. For Reynolds numbers greater than 100,000 the ratio is less than one fourth. An approximate Reynolds number for water at 55° F is 76,500 V D—in which V is the velocity, in feet per second; and D is the diameter, in feet. It should be remembered, however, that the viscosity of water is doubled by lowering the temperature from 85° F to 37° F, thus changing the Reynolds number from 114,000 V D to 57,000 V D.

TABLE 1.—TEST DATA FROM 2-IN. STANDARD BLACK PIPE

Velocity, v (ft per sec) (1)	Measured head, h_f (ft) (2)	Reynolds number, R (3)	Friction factor, F (4)
27.4	18.90	306,000	0.0276
25.3	16.50	282,000	0.0281
23.5	13.90	262,000	0.0277
21.75	11.95	248,000	0.0277
19.40	9.51	208,000	0.0277
16.50	6.99	174,000	0.0280
12.05	3.70	125,000	0.0277
8.88	2.01	106,000	0.0279
7.00	1.25	93,000	0.0279
6.24	0.990	83,500	0.0278
5.46	0.750	73,000	0.0277
4.15	0.435	55,500	0.0277
3.22	0.265	43,000	0.0278
2.65	0.176	35,500	0.0274
2.24	0.125	30,000	0.0274
1.92	0.0925	25,700	0.0274
1.734	0.0770	23,200	0.0280
1.623	0.0665	20,700	0.0278
1.440	0.0530	17,500	0.0281
1.332	0.0450	16,200	0.0278
1.121	0.0333	14,300	0.0290
1.015	0.0280	12,400	0.0298
0.874	0.0217	11,100	0.0313
0.737	0.0162	9,440	0.0324
0.601	0.01145	7,700	0.0345
0.395	0.00535	5,140	0.0375
0.280	0.00291	3,580	0.0405
0.232	0.00185	3,160	0.0376
0.1915	0.001083	2,620	0.0324
0.1677	0.000713	2,300	0.0277
0.1406	0.000598	1,920	0.0331
0.0915	0.000370	1,320	0.0486

A Typical Test.—All three of the modes of flow appear in the following test of a moderately rusty 2-in. standard black pipe. The test specimen used for this experiment was 10.0 ft long with an average diameter of 2.039 in., preceded by another 10 ft of the same 2-in. pipe. Conditions during the test were kept as nearly constant as practicable, with the exception of the velocity which was varied by a gate valve placed at a remote point. Small friction heads were measured with a tilting mirror gage developed for the purpose. The results are shown in Table 1. The symbol f, as usual, represents the friction factor to be applied through the Chezy principle by the Darcy-Weisbach formula, $h_f = f \frac{L}{D} \frac{V^2}{2g}$—this form seeming to have returned permanently to the engineering profession with its rational embodiment of pure dynamic laws.

The expanded form of the Reynolds number is $\mathbf{R} = \frac{V}{\mu}\frac{D}{}\frac{w}{g}$—in which μ is the absolute viscosity (preferably in pound-seconds per square foot); and w is the unit weight of fluid.

Three Methods of Presenting the Same Data.—The results of this test are plotted in three forms for reasons that will become obvious. The graph shown in Fig. 1 is drawn to natural scale to indicate relative values and thus give

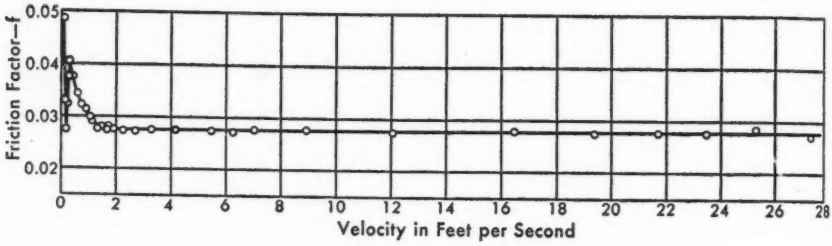

FIG. 1.—NATURAL-SCALE PLOTTING OF DATA FROM TEST ON 2-IN. STANDARD BLACK PIPE, 5 YEARS IN SERVICE

proper weight to the higher velocities where the friction factor f remains constant. This range in which the pure law is undistorted by viscous resistance is much greater than is suggested by the conventional logarithmic chart. Fig. 2 is a logarithmic plotting of the data shown in Fig. 1. The distortion is too often overlooked through a desire to magnify the smaller values. The same data are again shown in Fig. 3. This latter logarithmic plotting (loss of head with respect to velocity) is a still more dangerous expedient, particularly for extrapolation. The small break in the line of turbulent flow near the midpoint of Fig. 3 should be noted. By this method, the line is often carelessly drawn straight, with a slope depending on the particular range of velocities observed.

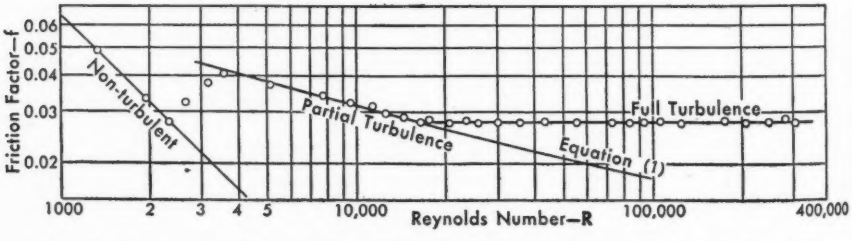

FIG. 2.—LOGARITHMIC SCALE OF DATA IN FIG. 1, REYNOLDS NUMBER REPLACING VELOCITY

In general, any plotting of data that leaves obscured within the scale such a major law as that of Chezy dwarfs the features that should be emphasized. When reviewing old experiments, it should be noted that an excessive number of low velocity readings have been recorded. These readings were readily secured, often to an abnormally low value, whereas those equally above the needs were probably not attainable. Had the data in Fig. 3 ended at 1.5 ft per sec, the error from producing the curve to 27 ft per sec would have been about 50%.

The choice of one or another of the three forms of plotting is one of emphasis, since the same facts are simply viewed in different perspective. All these forms show that in the higher velocities the loss is proportional to the square of the velocity, that is, the friction factor f is constant in response to the dominant law.

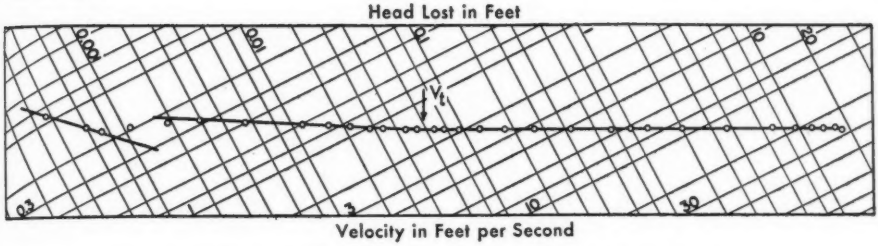

FIG. 3.—LOGARITHMIC SCALE OF VELOCITY AND HEAD LOST FOR DATA OF FIG. 1

At intermediate velocities, where the loss responds in part to the major dynamic law and in part to the various influences of viscosity, the lost head does not vary with any constant power of the velocity. It is customary and proper to use the Chezy principle (the Darcy-Weisbach formula) here, however, since the loss varies more nearly with the square of the vleocity than with the first power. The exponent shown in Fig. 3 is the common 1.75 in this middle range, which is equivalent to the value of f varying as the -0.25 power of the velocity. This exponent is known to apply only at the lower Reynolds numbers, and a more accurate formula will be given subsequently for all Reynolds numbers, including those less than 100,000.

At velocities low enough for the flow to remain nonturbulent, the dynamic law is replaced by an equally exact but entirely different law. Solved for drop in pressure, the fundamental Poiseuille formula is $p = \frac{32\,V\,L}{D^2}\mu$; and, since no f is required, it is regrettable that a fictitious f is so often used to circumvent this correct formula. The subterfuge, a method of including all velocities and their respective coefficients in a single graph, serves mainly for detecting the change from nonturbulence to partial turbulence. The implication of physical meaning is quite misleading since this highly variably f (where in reality there is no f) is simply the mathematical consequence of using a wrong formula. Nonturbulent flow being independent of the roughness of the pipe, the Poiseuille formula is a complete answer; and its range of application is reasonably defined. Although by providing local excitation one may secure partial turbulence factors at Reynolds numbers of 1,500 or less, under most circumstances the turbulence is damped out as rapidly as it is produced with Reynolds numbers even greater than 2,000. A Reynolds number of 2,000 is a reasonable lower limit of partial turbulence, however, since (for the purpose of computing losses) it is on the safe side.

In Fig. 2 there is no complex rising transition sometimes thought to be necessary for Reynolds numbers less than approximately 300,000, for this particular roughness. On the contrary a faint suggestion of a dip persistently reveals itself in this natural pipe.

The Transition from Partial Turbulence to Full Turbulence.—When there is a gradual transition between the two types of turbulent flow, it is usually explainable. Friction factors in the vicinity of the change from partial turbulence to full turbulence may be somewhat above the constant f if the total resistance is the result of two or more segregated degrees of roughness, or is somewhat below the constant f if the surface is roughened with a uniform size of elements. Further explanation of the dip caused by uniformity of artificial roughness will appear later.

An illustration of the gradual transition caused by two degrees of roughness that are well separated in the same pipe appears in the results of the following test. Unlike the effect of localized losses, which is to increase f an equal amount at virtually all velocities, this transition begins with the lowest velocity that supports full turbulence in the rougher section. A 20-ft length of 2-in. pipe was tested throughout a range of velocities with the results shown in the middle line of Fig. 4. A decided transition was apparent. The pipe was then cut into two 10-ft sections and each section was tested separately. By chance, one section proved much rougher than the average of the entire pipe; the rougher section showing practically no transition. Had the separate friction factors for each pipe been known, the graph of the 20-ft length could have been made in advance of the test, since it is simply the mean of the two halves.

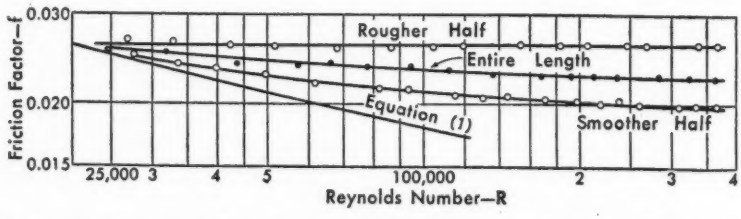

FIG. 4.—COMPONENTS OF TRANSITION FROM PARTIAL TURBULENCE TO FULL TURBULENCE IN 2-IN. NATURAL ROUGH PIPE

It is important to note that these gradual transitions can be reproduced by a properly weighted average of the constant f of the rough part of a pipe and the variable f of a perfectly smooth pipe. The two 10-ft lengths of pipe in the foregoing experiment were repeatedly tested together in all combinations of direction of flow and of connection one with the other, the graph of f remaining substantially as shown. This type of transition is not uncommon. New iron or steel pipe installed in the conventional manner may show its effect. The general surface is smooth enough to remain in partial turbulence at all ordinary velocities, but there are fully turbulent areas.

INADEQUACY OF SOME EARLY METHODS

The physical implications of the Chezy formula should not be treated lightly by empirical methods. Some of the older formulas ignore these rational features, with serious consequence. It is interesting to speculate as to what might have been Darcy's final conclusion had his elaborate program not been interrupted by his death in 1858, for he was in the midst of an intensive study

of flow along prepared surfaces having artificial roughness. His assistant, H. Bazin, was dominated by E. Ganguillet and W. R. Kutter[4] who attempted to evaluate the partly completed work, and neither of these men showed an eagerness for physical meaning in their mathematics.

No influence during the nineteenth century reacted more strongly against clear thinking than did Kutter's failure to distinguish between the two types of turbulent flow. Only his lower velocities or smaller conduits were in partial turbulence; yet the data were averaged indiscriminately. This weakness permitted such fallacies as that of the coefficient being always a function of the slope, and that of the rate of change in the coefficient being independent of relative roughness. Robert Manning[5] removed the former fallacy but retained and emphasized, in his formula, the latter and more serious fault. Fortunately the use of these old formulas is becoming less prevalent for pipes. The growing tendency to segregate coefficients into pipe coefficients, f, and channel coefficients, C, should be encouraged. Using C only for open channels with the formula $V = C\sqrt{Rs}$ prevents such features as shape of cross section and wave action from marring the coefficients to be used for pipes.

Bazin maintained[6] that the coefficient C did not normally change with changing slope, a fact entirely consistent with a constant f for those surfaces and velocities that were pertinent to his field of application. There the principle of dynamic similarity was undisturbed by viscosity. Despite its lack of dimensional consistency the Bazin formula does supply what Manning's formula does not—a greater percentage of change in coefficient for rough surfaces than for smooth surfaces, for any given change in size of conduit.

Weisbach,[3] being impatient with theories, was prone to discourage any rational approach unless quickly confirmed by superficial experiment. His methods did not tend to explain why his f varied. Thus it is not surprising that in the half century following, a few experiments were considered sufficient to establish a "law." The constant coefficient with fractional exponent like that which led to the formula of H. Blasius[7] originated in this atmosphere. The fractional exponent carried to this extreme obscures the rational elements, and makes the number of formulas unlimited.

At the beginning of the twentieth century the method of constant fractional exponent was at its zenith. One small volume that found its way to almost every library shelf, "Hydraulic Tables" by Gardner S. Williams and Allen Hazen,[8] Members, ASCE, was based on a fallacy that should have been apparent at the time. A constant-exponent formula was developed, selecting only the data from the smoothest pipes attainable, the exponent of V reducing to 1.85. It was then assumed that all rough pipes would follow that same exponential "law," the coefficient depending only on the degree of roughness.

[4] "A General Formula for the Uniform Flow of Water in Rivers and Other Channels," by E. Ganguillet and W. R. Kutter, translated by Rudolph Hering and J. C. Trautwine, Jr., John Wiley & Sons, Inc., New York, N. Y., 1889.

[5] "On the Flow of Water in Open Channels and Pipes," by Robert Manning, *Transactions*, Inst. of Civ. Engrs. of Ireland, Vol. 20, 1891, p. 161, and Vol. 24, 1895, p. 179.

[6] "Étude d'une Nouvelle Formule pour calculer le Debit des Canaux Découverts," by H. Bazin, *Annales des Ponts et Chaussées*, 4th Trimestre, 1897.

[7] "Das Ähnlichkeitsgesetz bei Reigungsvorgängen," von H. Blasius, *Physicalische Zeitschrift*, Vol. 12, 1911, p. 1175.

[8] "Hydraulic Tables," by Gardner S. Williams and Allen Hazen, John Wiley & Sons, Inc., New York N. Y., 3d Ed., 1920.

Fig. 5 illustrates the consequence. The graph shows the formula applied to a 9.63-in. pipe with C_w equal to 90. The test points are from an experiment by Darcy on an old cast-iron pipe as compiled by J. T. Fanning,[9] M. ASCE, and made available in English. Although it is approximately correct for lost head at a velocity of 1.5 ft per sec, the formula with the same coefficient is 26% in error at 12.6 ft per sec. This is not an isolated case. Fanning[9] published the results of other tests that showed the same characteristic. The Chezy formula with a constant f would have served these pipes with no appreciable error at any reasonable velocity.

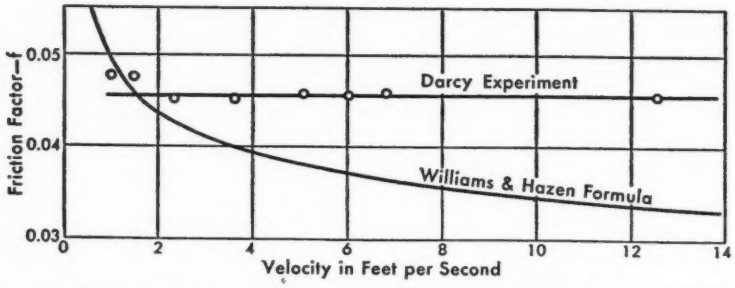

FIG. 5.—FAILURE OF WILLIAMS AND HAZEN FORMULA FOR A ROUGH PIPE TESTED BY DARCY

ARTIFICIAL ROUGHNESS VERSUS NATURAL SURFACES

Uniform Sand Grains.—Attention has frequently been called to the failure of experiments[10] by J. Nikuradse on uniformly roughened pipes to conform to experiments on ordinary pipes. The cause and the meaning of this disagreement are more apparent if one considers Nikuradse's work as an intensive study of the several components of one average degree of roughness. A composite of all his experiments conforms reasonably well to the pattern found for a single natural surface of this average roughness.

The solid line of Fig. 6 shows Nikuradse's work reduced to one graph for equal Reynolds numbers. The dotted line is his individual graph for approximately that average degree of roughness. The composite graph remains within 3% of a constant 0.035 for all Reynolds numbers above 12,000. Nikuradse's equivalent individual graph varies from 4% above to 14% below that value.

In this connection, one might examine a frequently quoted experiment by E. W. Schoder and H. A. Gehring,[11] on a 2-in. rough pipe. It shows practically a constant f of 0.034 for velocities ranging from 17 ft per sec to 4.6 ft per sec, well into the region of the Nikuradse dip; but, instead of a dip, there is simply a negligible tendency to rise.

The disagreement between random and uniform roughness cannot be

[9] "Practical Treatise on Hydraulic and Water Supply Engineering," by J. T. Fanning, D. Van Nostrand Co., 13th Ed., 1896, p. 240, Table 59.

[10] "Laws of Fluid Flow in Rough Pipes," Pt. 2, by J. Nikuradse, English translation in *The Petroleum Engineer*, May, 1940, p. 78, Fig. 6.

[11] "The Flow of Water in Spiral Riveted and Other Pipes," by E. W. Schoder and H. A. Gehring, *Engineering Record*, Vol. 58, 1908, No. 9, p. 240.

avoided. A dip in the graph must occur when all elements require the same range of velocities for the minimum resistance factor that precedes their fixed pattern of flow. It is a well-known fact that even spheres, in a certain range of Reynolds numbers, are streamlined by the viscous fluid, mainly behind them; but it is not necessary to visualize the influence of individual elements. These elements are all shielded by the boundary layer until the velocity is sufficient to expose them. The guiding boundary, as modeled crudely through the nonturbulent fluid, becomes rough enough to cause pure turbulence when it is still smoother than the solid wall itself. When stronger vortices of turbulence within the shielded depressions deplete the remaining nonturbulent fluid, the flow is controlled by the solid elements. The fixed pattern is thus established and the friction factor has risen to its normal constant.

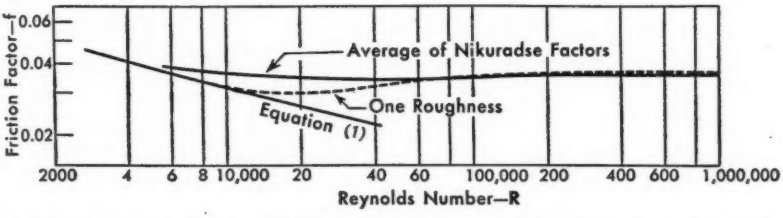

FIG. 6.—ONE UNIFORM ROUGHNESS VERSUS THE AVERAGE OF SIX DEGREES OF ROUGHNESS

Since the tendency to dip occurs at higher velocities for the smaller elements, it follows that only when many roughness elements are of one size does the dip become perceptible; and, since each size of roughness element contributes only a minor tendency, the integrated effect does not materially lower the graph as a whole. Even if the lowering were appreciable, no correction would be necessary, since the selected value must in any case come from a natural pipe possessing that quality.

The most logical procedure therefore is to assume the perfect dynamic law with its horizontal graph, and to modify f only if a reason exists—rather than to supply some variable general function presumed to be required, and on most occasions to reduce it to a constant. The constant f is a complete designation of the relative roughness of a pipe; so it would be unfortunate indeed if an artificial surface with a maximum departure from the pure law should be chosen as standard for comparing other surfaces that in general show no such departure.

Artifically Prepared Random Roughness.—To simulate the inside surface of a rough pipe and at the same time to insure that pipes of various diameters would present the same absolute roughness, the writer outlined an investigation based on artificial linings. The material selected was No. 20 sheet steel, on which was sprayed a uniform coating of stainless steel. Large sheets were prepared in this manner with a controlled adjustment, and the various linings were cut from these sheets. The lining for each of the five pipes was annealed and formed by a special die so as to fit inside its respective standard pipe. For sizes of $\frac{1}{4}$ in., $\frac{1}{2}$ in., 1 in., and 2 in. (iron pipe size before lining), brass pipe was used for the casing. The 4-in. size was made to fit 4-in. (inside diameter)

Shelby tubing. The predominating length of lining was 2 ft and the total length of each test specimen was 60 diameters for the largest size, and 100 diameters or more for the other sizes.

The characteristics of this surface were entirely satisfactory for the purpose. The random nature of the roughness elements was sufficient to prevent most of the dip at the beginning of full turbulence, the ¼-in., 1-in., and 4-in. sizes showing substantially none. The fact that the roughness was well-distributed longitudinally along the pipe prevented a long transition. The results were in close conformity with those for moderately rusty iron pipes. The surface was somewhat rougher than that of the smoothest rusty pipe, but decidedly smoother than the Schoder 2-in. rough pipe previously mentioned and even smoother than the rusty pipe of Fig. 1. The results taken from the 4-in. size are shown in Fig. 7. The other four pipes showed approximately the same characteristics. The probable deviation for an individual surface is 2.1%. This type of surface also serves to relieve the impression that there is merit in knowing the exact size or shape of the roughness particles.

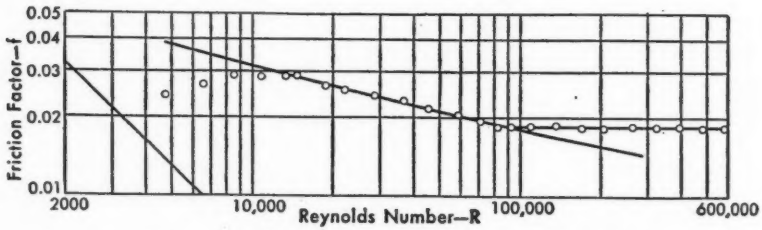

FIG. 7.—ARTIFICIAL RANDOM ROUGHNESS IN A 4-IN. PIPE

In keeping with the nature of rough and moderately rough iron pipes, the change from partial turbulence to full turbulence came at substantially the same velocity for all diameters, when corrected proportionately for viscosity. The breakaway with this particular surface came at somewhat less than 4 ft per sec. This break will be shown to correlate with the roughness coefficient.

A SMOOTH PIPE FORMULA AND ITS DOUBLE PURPOSE

The need for a simpler method of finding the resistance to flow in smooth pipes gained added emphasis through the fact that flow in a rough pipe conforms so closely to that same law within the range of partial turbulence. A single formula for f, when properly adjusted for localized losses, thus serves for all smooth pipes, and also for rough pipes if the velocities are less than the velocity which causes full turbulence.

A number of simple formulas is available within a given range of Reynolds numbers; but the attempt to extend the range indefinitely by rationalizing as in some treatments has produced unnecessary complexities. Such methods still include empirical steps which prevent a purely rational interpretation. In this respect, one might question the validity of assuming that an infinite Reynolds number would be accompanied by a zero friction factor, since this assumption reacts against simplicity. Consider the use of various fluids with

progressively decreasing viscosity. The flow with each higher Reynolds number is of course more turbulent than with the preceding one, yet the layer of fluid in contact with the wall is still interlocked mechanically with the molecular structure. The turbulent flow in contact with either this boundary layer or the surface itself furnishes the condition for impact without compensating reflection. The resistance cannot therefore approach zero with the approach of zero viscosity. A similar conclusion is reached for those large Reynolds numbers that result from large physical dimensions. Since f is the loss of head (in terms of velocity head) that results from a length of pipe equal to one diameter, the length approaches infinity in the same manner that the diameter approaches infinity. Thus the abstract f here also approaches some positive finite value. This limiting value is dependent only on the fact that the fluid attaches itself to the walls of the pipe.

One should be reluctant to discard the advantages of the type of formula recommended by Charles H. Lees[12] $\left(f = A + \frac{B}{\mathbf{R}^n}\right)$, provided the exponent n and the other constants A and B are correct for the required range. Within the range of present knowledge (which extends up to a Reynolds number equivalent to a velocity of 5 ft per sec in an 8-ft pipe), the variable part of f may be assumed to change inversely with the cube root of the Reynolds number. Whether or not this condition extends to the theoretical limit is not pertinent to practical needs. The important fact is that this simple function is correct throughout the usable range and approaches 0.0061 as a limit.

The experimental data that led to the following formula were observed in connection with the calibration of some equipment installed in 1945 at the laboratory of the University of Washington in Seattle. The pipe was of brass, 19.5 ft long with an average inside diameter of 2.050 in. The water was weighed in a calibrated tank scale and the head was measured by open columns of water and mercury. The results are presented in Table 2. The Blasius "line" having been so repeatedly confirmed at a Reynolds number of 10,000, this one point was retained and the new data were accepted for the higher Reynolds numbers, ranging from approximately 100,000 to more than 1,000,000.

TABLE 2.—TEST DATA FROM A 2-IN. SMOOTH PIPE

Velocity (ft per sec)	Measured head (ft)	Reynolds number, $\mathbf{R}$	Friction factor, f	Gage used
7.02	1.55	105,000	0.0177	Water
10.70	3.29	156,000	0.0162	Water
15.20	6.21	221,000	0.0152	Mercury
22.49	12.60	328,000	0.0141	Mercury
32.75	25.15	476,000	0.0132	Mercury
47.51	49.6	693,000	0.0124	Mercury
62.97	83.4	916,000	0.0118	Mercury
69.43	99.3	1,040,000	0.0116	Mercury

With $f = 0.0316$ for a Reynolds number $\mathbf{R}$ of 10,000 and the observed values of f taken from Table 2,

$$f = 0.0061 + \frac{0.55}{\sqrt[3]{\mathbf{R}}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

The advantage of a pure cube root from the standpoint of easy application is obvious, and the rational aspect may be more than implication.

[12] "On the Flow of Viscous Fluids Through Smooth Circular Pipes," by Charles H. Lees, *Proceedings*, Royal Soc. of London, Series A, Vol. 91, 1915, p. 46.

Even more important than its discovery through a simple experiment, however, is the agreement of Eq. 1 with other reliable data. Intersecting the Blasius line at a Reynolds number of 10,000, the graph follows closely below that line, recrossing it at 49,000. Continuing throughout the data that supported T. E. Stanton and J. R. Pannell,[13] it agrees with T. B. Drew, E. C. Koo, and W. H. McAdams[14] who included in their study the noteworthy experiments on brass pipes by John R. Freeman,[15] Past-President and Hon. M. ASCE. It also conforms to the Nikuradse experiments[16] up to 3,200,000, within a normal error of those points. These facts are easily verified with greater refinement than necessary. It is not more precise information that is needed, but a more workable method of interpreting that already at hand. (No comparison is made with the Nikuradse formula itself. To secure the small exponent of 0.237 one must ignore the disagreement at low Reynolds numbers and assume an agreement, at points beyond physical verification, with a formula that makes f approach zero with the approach of an infinite Reynolds number. When reaching beyond the range already examined, it would seem unwise to sacrifice expediency and to retain a feature even mildly against safety.)

In addition to the obvious purpose of Eq. 1 for smooth pipes and for partly turbulent flow in rough pipes, its graph also furnishes the stem from which the constant full turbulence factors spring, as shown in Fig. 2, and subsequently in Fig. 9. If there is a transition, the junction point can be located more definitely by extending the horizontal line back from the higher velocities where it is firmly established. This point on the graph of smooth pipes thus marks the division point between partial turbulence and pure turbulence of a rough pipe.

Solving for the Reynolds number in Eq. 1, and knowing that complete turbulence is indicated by the appearance of a constant factor, one finds the Reynolds number $\mathbf{R}_t$ at which pure turbulence is pending:

$$\mathbf{R}_t = \left(\frac{0.55}{f_t - 0.0061}\right)^3 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

in which f_t is the constant f for full turbulence.

Rough Pipes of Same Material and Various Diameters

A method of correlating pipes made of the same material, and thus of evaluating the influence of relative roughness, is furnished through the fact that the excitation for full turbulence depends on the size and the shape of the pattern of roughness rather than on the diameter of the pipe. Eq. 1 is based on the pipe diameter, whereas the break to full turbulence responds to the Reynolds number of the pattern. Hence, no numerical value is required for this Reynolds number of the pattern. The only essential fact is that it re-

[13] "Similarity of Motion in Relation to the Surface Friction of Fluids," by T. E. Stanton and J. R. Pannell, *Philosophical Transactions*, Royal Soc. of London, Series A, Vol. 214, 1914, p. 199.

[14] "The Friction Factor for Clean Round Pipes," by T. B. Drew, E. C. Koo, and W. H. McAdams, *Transactions*, A. I. C. E., Vol. XXVIII, 1932, p. 56.

[15] "Experiments Upon the Flow of Water in Pipes and Pipe Fittings Made at Nashua, New Hampshire, June 28 to October 22, 1892," by John R. Freeman, A. S. M. E., New York, N. Y., 1941, p. 83.

[16] "Gesetzmässigkeiten der turbulenten Strömung in glatten Rohren," by J. Nikuradse, *Forschungsheft*, Verein Deutscher Ingenieure, 356, September–October, 1932, p. 30, Table 9.

mains constant with changing diameter. Therefore, with the constant friction factor known for one diameter, the corresponding factor for any other diameter with this same pattern of roughness can be taken from Eq. 2, but with a Reynolds number of the pipe in proportion to the diameter. Accordingly, if the constant friction factor f_1 is known for a pipe whose diameter is D_1, the corresponding value of f_2 for a pipe whose diameter is D_2 is $f_2 = 0.0061 + \dfrac{0.55}{\sqrt[3]{\dfrac{D_2}{D_1}}\,\mathbf{R}_1}$;

and, since the controlling Reynolds number $\mathbf{R}_1$ for the factor f_1 is $\mathbf{R}_1 = \left(\dfrac{0.55}{f_1 - 0.0061}\right)^3$, an expression results which is independent of the Reynolds number: $f_2 = 0.0061 + \dfrac{f_1 - 0.0061}{\sqrt[3]{\dfrac{D_2}{D_1}}}$.

Defining the friction factor f_1 as the specific roughness coefficient for a unit diameter of 1 ft, the foregoing formula becomes

$$f = 0.0061 + \frac{f_1 - 0.0061}{\sqrt[3]{D}} \quad \ldots (3)$$

in which D is the diameter, in feet; and f is the friction factor to be used with that diameter. A test of any size of pipe having a given character of surface reveals the same value of f_1.

Uses of the Specific Roughness Coefficient.—Although no experiments on rough pipes were required in the derivation of any of the foregoing formulas, quantitative values of f_1 must be derived from tests made on actual surfaces.

The average specific roughness coefficient f_1 for the five pipes having the surface metallized with a spray of stainless steel was 0.0146, as given in Table 3. The five individual points, taken from the higher velocities only, are shown in Fig. 8. The graph is determined by Eq. 3 and gives the friction factors for all pipes made with that same interior surface.

TABLE 3.—TEST DATA FROM PIPES WITH SPRAYED STAINLESS STEEL LININGS

Velocity (ft per sec)	Degrees centigrade	Friction Factors: f	Friction Factors: f_1
(*a*) Diameter D = 0.294 In.			
30.2	18	0.0360	
37.0	17	0.0360	
43.7	17	0.0360	0.0148
(*b*) Diameter D = 0.542 In.			
24.0	14	0.0281	
25.8	13.5	0.0281	
30.8	13.5	0.0281	
36.6	13	0.0287	
41.3	13	0.0281	
45.4	13	0.0282	0.0140
(*c*) Diameter D = 0.977 In.			
29.7	10	0.0247	
32.8	10	0.0246	
36.9	10	0.0247	
41.5	9	0.0247	
45.5	9	0.0248	
48.5	9	0.0247	0.0142
(*d*) Diameter D = 1.955 In.			
32.8	8	0.0229	
36.6	9	0.0232	
40.4	9	0.0233	
45.9	9	0.0233	
50.6	9	0.0234	
41.7	20	0.0224	
44.9	20	0.0228	
47.6	20	0.0232	0.0154
(*e*) Diameter D = 3.898 In.			
....	For data see Fig. 7		0.0145
....	Average for five diameters		0.0146

The identification of this surface by its specific roughness coefficient is further illustrated in Fig. 9. The horizontal line indicates the average of the

foregoing five specific roughness coefficients. The points are the individual specific coefficients computed from fifty-three observations, ten or more on each of the four larger pipes. (The respective diameters were 0.541 in., 0.976 in., 1.955 in., and 3.899 in. Flow in the fifth pipe with a diameter of 0.294 in. was nonturbulent below a Reynolds number of 6,000.) All velocities were adjusted to a temperature of 55° F, making the velocity for full turbulence equal to 3.5 ft per sec. The inclined line is computed by Eq. 1 for smooth pipes.

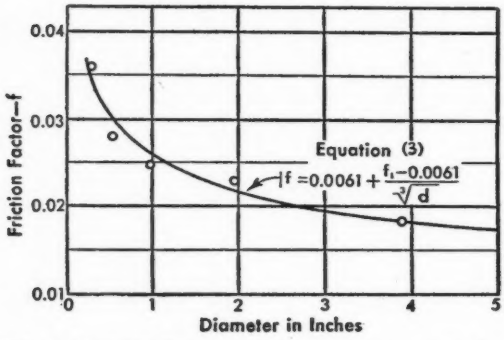

FIG 8.—FRICTION FACTORS FOR FULL TURBULENCE IN FIVE DIAMETERS WITH RANDOM ROUGHNESS
(Correction: In the formula for f, change d to D)

The average specific roughness coefficient, as computed by Eq. 3 for Nikuradse's smallest sand grains that were used in all three of his pipes, is 0.0152. These three points, as shown in the lower line of Fig. 10, fall reasonably on the graph. Nikuradse's experiments with the largest sand grains reveal the average specific coefficient of 0.0277 shown in the upper line. The effective diameter of the smallest pipe when used with the largest sand grains is sensitive to the size of roughness particles. To assume a section of average area rather than a clear opening, in the extreme limit, may result in a negative sectional area for a positive diameter. If the radius were assumed smaller by even one fourth of the diameter of the sand grains, that point would be materially lowered.

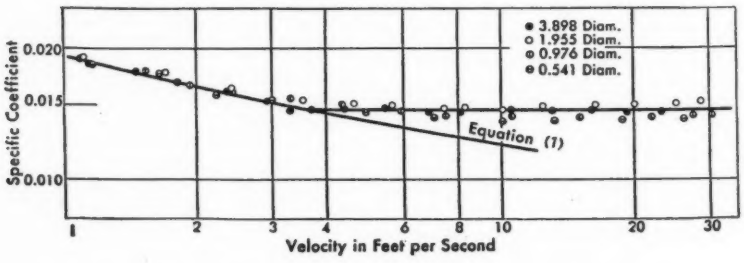

FIG. 9.—CORRELATION OF ADDITIONAL TESTS WITH THE AVERAGE FOUND FROM HIGHER VELOCITIES AS USED IN FIG. 8

The Index of Full Turbulence.—Solving Eq. 2 for the V_t that is embodied in the Reynolds number and preserving the relation of f and D as in the derivation of Eq. 3, one finds the index velocity for full turbulence:

$$V_t = \nu \left(\frac{0.55}{f_1 - 0.0061} \right)^3 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4)$$

As usual ν is the kinematic viscosity obtained by dividing the absolute viscosity μ by the mass density w/g. (The kinematic viscosity of water at 55° F is 0.00001307 sq ft per sec.)

Since the roughness coefficient f_1 is the same regardless of the diameter from which it is found, the index velocity V_t that fulfils the conditions for incipient full turbulence is independent of the diameter. This point is even more stable than the critical point that indicates nonturbulence. The velocity marked by the intersection of graphs in Fig. 9 can be computed by Eq. 4 using the average specific roughness coefficient. Its correlation with the observed points should be noted. When applying Eq. 4 one should make certain, of course, that the roughness factors do not include localized losses.

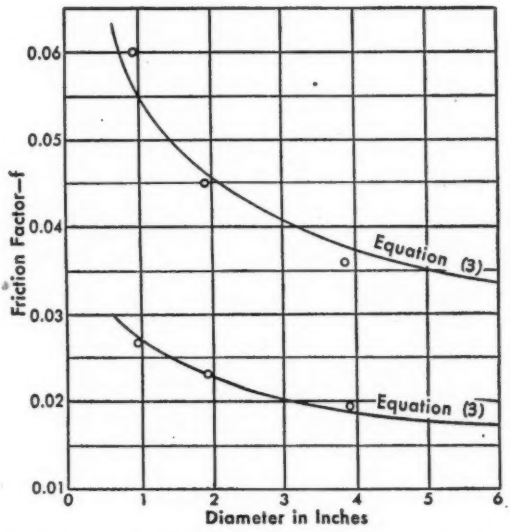

FIG. 10.—NIKURADSE'S THREE PIPES WITH TWO SIZES OF SAND GRAINS

Although Fig. 17 will be presented subsequently for rough pipes at velocities above V_t where friction factors are independent of velocity or temperature, it could be used in comparing pipes at the same velocity whether they are rough or smooth. The specific coefficients in Fig. 9 for example were all adjusted to a diameter of 1 ft by Eq. 3 whether they were the constant roughness factors of full turbulence or the variable factors in partial turbulence. Plotting these latter points according to the velocity instead of the Reynolds number keeps them on the inclined graph because of the relation of Eqs. 3 and 1. When adjusting for a significant temperature difference observe that the velocity at which these variable friction factors are correct is in proportion to the viscosity.

If V_t is greater than the highest velocity ever to be used for pipes with some particular surface, those pipes act as smooth pipes, and making them smoother will not increase the flow. The friction factor should then be taken directly from the smooth pipe formula. On the other hand, in very rough pipes, V_t is often so small that partial turbulence ceases to be a significant feature. If there is a question, try both methods and select the larger value of f.

Concrete drain tile presents a rough surface, particularly when the open joints are included. The irregularity of manufacture and of placement makes this condition a severe test of a specific roughness coefficient. A series of experiments at the University of Iowa[17] at Iowa City serves to indicate the

[17] "The Flow of Water in Drain Tile," by D. L. Yarnell and Sherman M. Woodward, *Bulletin No. 854*, U. S. D. A., Washington, D. C., 1920, p. 1.

correlation in this extreme case. Fig. 11 shows the six friction factors measured for diameters of 4 in. to 12 in., inclusive, for velocities above the index for the surface alone. The graph is computed by Eq. 3 using the average of their specific roughness coefficients. The probable deviation for a pipe selected at random, even with all its crudity, is less than 7%.

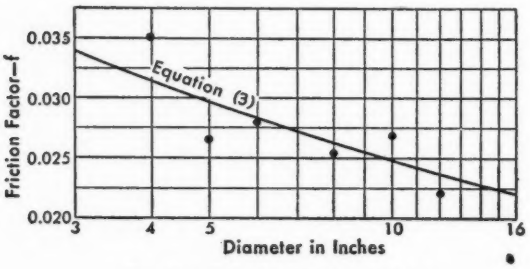

FIG. 11.—OPEN-JOINTED DRAIN TILE

Concrete Pipes.—The laws that control f being of universal application for models in general, the identification of a surface by its breaking away from the influence of viscosity is supplied by Eq. 1, and therefore may be widely extended by Eq. 3. Concrete pipes furnish an example of a large range of diameters, with approximately the same nature of surface. In 1920 an exhaustive study of these pipes was reported by the United States Department of Agriculture.[18] Nine years later Fred C. Scobey, M. ASCE, gave a digest of these findings for the American Water Works Association.[19] The pipes in this latter report

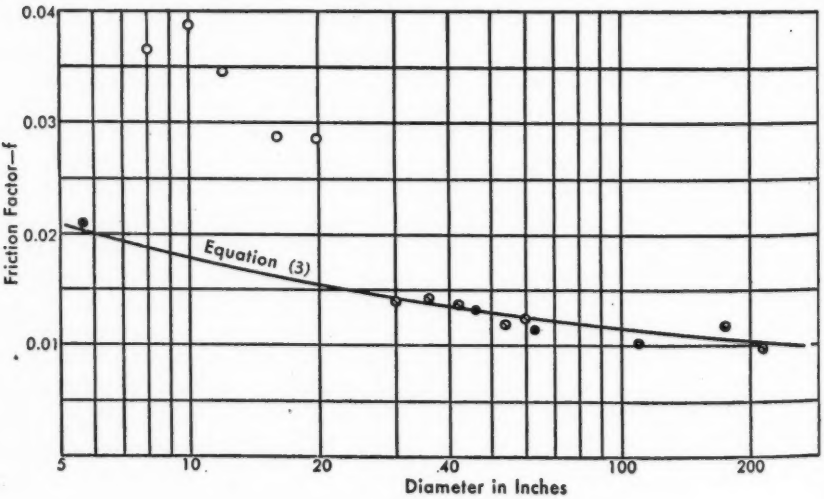

FIG. 12.—CONCRETE PIPE

are included in the following study except those described as probably containing deposits of sand or vegetable growth, short reaches questioned in the digest, jointed pipes less than 3 ft in diameter, and one old test by Fanning, having an insufficient description.

[18] "The Flow of Water in Concrete Pipe," by Fred C. Scobey, *Bulletin No. 852*, U. S. D. A., Washington, D. C., 1924, p. 95.

[19] "Coefficients of Flow in Concrete Pipe," by Fred C. Scobey, *Journal*, A.W.W.A., January, 1929, p. 1.

Mr. Scobey found the exponent of V to be substantially 2. By accepting this perfect square, one accepts a constant f for all velocities, and classifies the data as coming from pipes in pure turbulence.

The average specific roughness coefficient for these typical concrete pipes is 0.0171 ± 0.0002. Fig. 12 shows the graph for this specific roughness. The solid circles indicate cement lined pipe. The general agreement of the points with respect to the graph (the shape of which comes from Eq. 3) indicates the reliability of the data and of the method. Semilogarithmic plotting is used in Fig. 12 because the ratio of diameters is more important than their actual magnitudes.

The pipes included in this study do not necessarily represent materials and workmanship yet to be developed. They are chosen as typical examples to illustrate the use of a single specific roughness coefficient for all pipes of a given material.

It is well to note again why Kutter's n is larger for the larger pipes, inasmuch as n was conceived to be a factor remaining constant for all diameters. The formula, by making substantially the same percentage of correction for relative roughness whether the pipe be rough or smooth, overcorrects even these moderately smooth pipes. The result of this defect has usually been explained away by assuming the larger pipes to be actually rougher, an assumption generally contrary to fact. F. C. Finkle[18] has stated that the proper n for a 50-in. concrete pipe is 10% greater than that for an 18-in. pipe of the same workmanship. This 10% increase in n is no small matter. If applied to one of two pipes of the same diameter, it would mark that one for approximately 20% more loss. The variation of n being in reverse also leads to the erroneous conclusion that the larger a pipe is made the greater its roughness becomes. Kutter's formula serves no useful purpose for smooth surfaces, not even for concrete pipes. The n in this example varies as much as the C which it is supposed to correct. The Manning formula has this same characteristic.

Analysis with Eqs. 3 and 4 shows that the placing of concrete pipes in the category of full turbulence is justified. At ordinary temperatures with a specific roughness of 0.0173, the index velocity V_t from Eq. 4 is approximately 1.6 ft per sec.

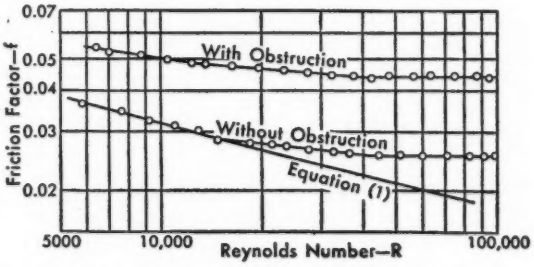

FIG. 13.—THE EFFECT OF AN OBSTRUCTION AT THE MIDPOINT OF A 20-FT LENGTH OF 2-IN. PIPE

Effect of Localized Losses.—When applying Eq. 4 it should be observed that losses resulting from obstructions or defects so widely separated as to act individually do not affect the index velocity. Friction factors as reported are not always free from such influence. Since these localized losses usually vary with the square of the velocity, they add a constant amount (natural numbers, not logarithmic) to the value of f whether the flow be partly turbulent or fully turbulent. The augmented graph should not be confused with a real transition

resulting from two or more degrees of roughness acting separately in the same length of pipe.

Fig. 13 shows the effect of an obstruction at the midpoint of a 2-in. pipe 20 ft long. The lower line is the graph of the pipe before the obstruction was introduced. The upper line is the result of a partly closed standard gate valve at this midsection. The important fact is that the surface loss remained substantially unchanged.

The average specific roughness coefficient for the five bell-jointed pipes, shown by plain open circles in Fig. 12, is 0.033. This value is approximately double that for continuous pipe. The index velocity V_i, however, responds more nearly to the surface roughness. This fact is confirmed by the higher values of f found below that velocity. The data on the drain tile previously mentioned include more low velocities and therefore show more definitely that the point of change corresponds to the specific roughness coefficient of continuous pipes.

Old cast-iron pipes that have become tuberculated with large, widely spaced blisters fall in the category of pipes with localized losses and should not be analyzed by Eq. 4 until a reasonable estimate of these losses has been made and the friction factors reduced by that amount; but, since these old pipes operate so predominately in full turbulence, V_i is not so important.

New Iron Pipes.—Black iron or steel pipes are usually listed under the old name, "wrought iron." Until these pipes have rusted, the friction factor for the greater part of the surface is the same as that for any other smooth pipe; but with ordinary defects of manufacture, including roughness along a welded rib, there is generally an appreciable area rough enough to be in full turbulence. The net result, therefore, is a long transition resulting from two separate degrees of roughness.

The experiments by Mr. Freeman[15] show a wide variety of surfaces in the new iron pipes of an early period. Despite this variation, his measured results can be secured by supplying approximately three fourths of the length with the smooth pipe coefficient and the remaining length with a specific roughness coefficient.

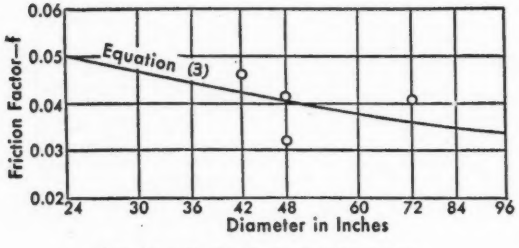

FIG. 14.—STEEL PIPE, 25 YEARS OLD

It is not so advantageous to have a smoother surface as it is to keep a larger percentage of the surface smooth. In other words, uniformity of surface and the prevention of corrosion are more important than high polish.

Old Steel Pipes.—The rate at which unpainted steel surfaces become rusty and tuberculated when exposed to water with various chemical impurities is beyond the scope of this paper; but an impression of what may occur in 25 years can be obtained from a description by Charles H. Capen,[20] M. ASCE, in 1941. Four pipes described by Mr. Capen, are: The 42-in. Pequannock

[20] "Trends in Coefficients of Large Pressure Pipes," by Charles H. Capen, *Journal*, A.W.W.A., January, 1941, p. 1.

conduit No. 2, the 48-in. Pequannock conduit No. 1, the 48-in. Pequannock conduit No. 2, and the 72-in. Rockaway conduit No. 1. Fig. 14 shows the friction factors of these pipes, interpolated for the age of 25 years. They exhibit irregularity, even for the two pipes of the same size, but clearly indicate a correlation with diameter. The specific roughness coefficient is 0.0617, indicating a loss of head approximately two and a half times that in a concrete pipe. Much of the indicated "roughness" may result from neglecting the change in diameter caused by internal surface deposits.

Wood Stave Pipes.—Wood staves present a surface that is reasonably the same for a wide range of diameters; but contradictions in published data cause one to hesitate in drawing general conclusions. Fortunately the presence of a localized loss does not change the general shape or the slope of the graph but simply lifts the graph to a higher level. When a small percentage of the length is in partial turbulence, the otherwise horizontal graph inclines only slightly; so an approximate analysis is possible.

An early impression that wood stave pipe remains smooth throughout a long life was furthered by placing too great emphasis on data from low velocities where considerable roughness makes no difference. There is unmistakable evidence that a specific roughness coefficient develops in wood stave pipes within a few months. With an approximate value of 0.015 in mind one should not expect any considerable departure from smooth pipe coefficients at velocities less than 3 ft per sec (3.08 ft per sec at 55° F).

Examination of the data compiled by Mr. Scobey[21] in determining his exponential formula reveals, among other things, the ill effect of vertical and horizontal curves in sequence in continuous stave pipes, as well as the ill effect of poor alinement in jointed pipes. Since these and other defects are so often present, there should be added to the coefficient for either partial or full turbulence an amount that may exceed 0.008. There are deviations from this value but it would seem unwise to cover poor workmanship or the effect of a disintegrated surface by a sweeping factor of safety.

Open Channels in Relation to Closed Pipes.—In the foregoing treatment of flow in pipes, all equations are referred to the diameter. Hydraulic radius is not used for pipes because it is an unsatisfactory correlating factor. For example, it is common knowledge that even a semicircular flume does not correspond to half of a closed pipe of that size.

Adaptation to open channels, in so far as pipes and channels agree, can be made by the well-known relationship between the Darcy-Weisbach f and the Chezy C. The identity of the equations, $h_f = f \frac{L}{D} \frac{V^2}{2g}$ and $V = C\sqrt{Rs}$, is too often overlooked. The equation $C = \sqrt{\frac{8g}{f}}$ is simply a means of conversion.

Although this paper is not one in which to expand the subject of open channels, the writer will not refrain from suggesting the use of a specific channel

[21] "The Flow of Water in Wood-Stave Pipe," by Fred C. Scobey, *Bulletin No. 376*, U.S.D.A., Washington, D. C., 1916.

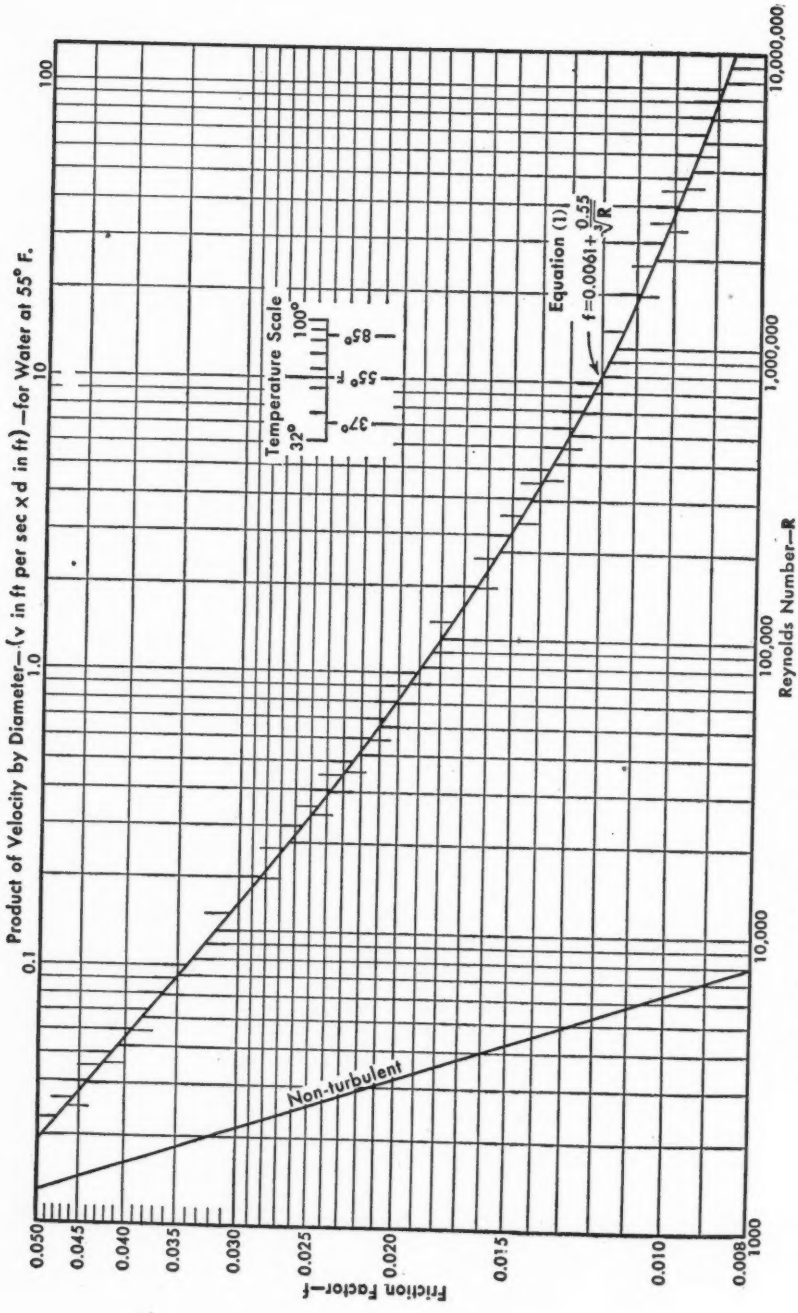

FIG. 15.—FRICTION FACTORS FOR ALL SMOOTH PIPES AND FOR OTHER PIPES IN PARTIAL TURBULENCE
(Correction: In the upper caption change v and d to V and D)

coefficient based on the hydraulic radius of 1 ft. The simple mathematics shown herein, when accompanied by a "shape factor," furnishes the method of application. With each surface identified by its specific coefficient, the need for a Kutter's or a Manning's n or a Bazin's m will disappear. The specific channel coefficient C_1 for a given surface is the Chezy C corresponding to the friction factor for a 48-in. pipe having that same surface. Of course, the C_1 should be found by measurements on open channels, in which case the error of reduction to round pipes is avoided. Each surface has its own channel coefficient C_1 and, other things being equal, the flow is proportional to it.

Diagrams for Smooth and Rough Pipes

A method of applying the foregoing treatment is incorporated in the following diagrams. Pipes fall into two categories: (1) Smooth pipes, or rough pipes below the index of pure turbulence; and (2) rough pipes in pure turbulence.

Smooth Pipes.—The graph of Eq. 1 in Fig. 15 serves all smooth pipes and all fluids whose viscosities are known—oil, air, water, etc. The upper scale of $V D$ is a convenience for use with water only. It reads directly for water at 55° F, and can be adjusted for other temperatures by shifting the upper grid an amount and direction indicated by the temperature scale shown on the chart.

Rough Pipes.—With velocities less than the index velocity V_t, as taken from Fig. 16, rough pipes require the same coefficients shown for smooth pipes. They are accordingly served by Fig. 15, provided any localized loss is added. A surface with roughness reasonably distributed does not of itself produce a localized loss.

Friction factors for velocities greater than V_t depend only on the diameter and the specific roughness coefficient of the surface. With these known, the desired constant friction factor is found in Fig. 17. This case covers a broad field of civil engineering in which the friction factor is not influenced by velocity or viscosity. This field is broadened because the controlling velocity for designing a pipe line is its highest velocity; and the specific roughness coefficient is generally adopted for future years.

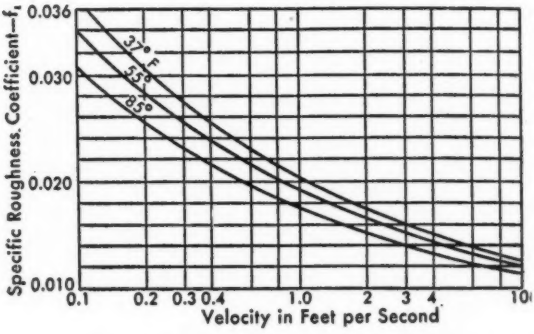

Fig. 16.—Influence of Temperature of Water on Velocity Producing Full Turbulence

A possible type of diagram that would include both rough pipes and smooth pipes could be prepared by superimposing the horizontal lines from Eq. 3 on the graph of Eq. 1. Making the interval between horizontal lines correspond to

the change in friction factors caused by successively doubling the diameter would give a diagram similar to the one usually shown for the Nikuradse data. No experimental information would be required in making this diagram for rough pipes. Experiments would only be necessary to make it applicable to specific surfaces. The dip would naturally be lacking, as it should be for random roughness. Such a method implies a need for a linear measure of the roughness pattern, and suggests the finding of a Reynolds number whether it is pertinent or not. It is simpler to depend on a friction factor measured for one diameter, and to proceed by relative diameters, as in Fig. 17.

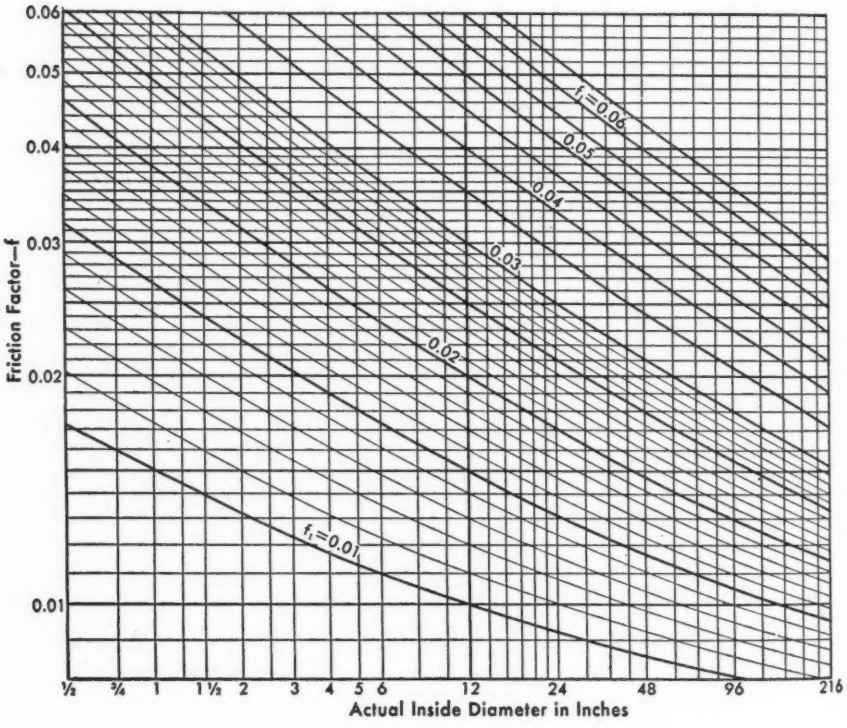

FIG. 17.—INFLUENCE OF DIAMETER ON FRICTION FACTORS IN FULL TURBULENCE

Specific Roughness Coefficients for Pipes.—The specific roughness coefficient f_1, being the constant friction factor for a 1-ft pipe, may be found from experiment on a pipe of any diameter by Fig. 17 or by Eq. 3. Material and workmanship will follow the trend of the times, and better measurements in the field will strengthen the accumulated data. In the meantime, the following typical examples will aid in securing a reasonable design:

Item	Material	f_t	V_t at 55° F
1	Metallized stainless steel in the rough	0.0146	3.5
2	Wood stave pipe, old surface	0.0150	3.0
3	Concrete pipe, straight continuous; also cement lined steel pipes	0.0173	1.6
4	Black pipe, 5 years old, surface only	0.018	1.5
5	Cast-iron pipe, bare surface	0.023	....
6	Wood stave pipe, continuous with horizontal and vertical curves, and jointed pipe with average alinement	0.023	3.0
7	Concrete drain tile	0.024	1.6
8	Cast-iron pipe, average for permanent city mains	0.028	....
9	Concrete pipe, 24 in. and under, bell jointed	0.032	....
10	Cast-iron pipe, old and foul	0.042	....
11	Riveted steel, 25 years old and tuberculated	0.062	....

(In items 6 and 7, the turbulence index V_t responds to the surface roughness, not to the localized losses.)

Concluding Remarks

The rational aspects of flow in pipes deserve more confidence than has usually been given them. The arguments presented in the preceding pages have accordingly been directed by the relation of topics and not by the chronological order in which the factual material has appeared in print. Any simplification has come from adhering closely to the basic physical laws rather than from attempting to circumvent those laws by individual experiments. Much of the complexity that so often results from meticulously examining too narrow a field can thus be avoided.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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PAPERS

PHOTOGRAMMETRY, POSTWAR

By Leon T. Eliel[1]

Synopsis

Photogrammetry developed at a normal, cautious pace in the decade before World War II and had only achieved a certain minor acceptance among engineers in general. The war gave such a spectacular demonstration of the many applications of aerial photography and photogrammetry that engineers everywhere have grown to recognize these new tools as something very practical and ready for immediate use in their problems. This paper concerns the status of the art of aerial photography and photogrammetry as influenced by wartime experience.

By 1941 the aerial cameras used during the war had already been developed to a large extent. The only thing that the war did was to take some of these cameras and apply them in a new way. Photogrammetric methods also had been producing contour maps successfully in the United States for a period of more than ten years. Although acceptance was far from universal, many engineers had used the photogrammetric contour map for topography in dam design, reservoir studies, highway location, geological problems, pipe line location, transmission line location, city planning, real estate developments, and a great number of other problems.

The Tennessee Valley Authority (TVA) was the first agency of the federal government to foster photogrammetric topography on an extensive scale, beginning in 1937. The Geological Survey (United States Department of the Interior) was using the photogrammetric method for producing a substantial part of its total topographic program by 1941. The Coast and Geodetic Survey (United States Department of Commerce), the Forest Service (United States Department of Agriculture), the Engineer Department (United States Army), the Soil Conservation Service (United States Department of Agriculture), and the Hydrographic Office (United States Navy) were all making extensive use of the new methods.

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by **October 1, 1949.**

[1] Vice-Pres., Fairchild Aerial Surveys, Inc., Los Angeles, Calif.

Considerable experimentation had been made in color prior to World War II, and color had developed to a point where very satisfactory work was being done from the air, although the control of tone and contrast has proved difficult.

The main developments that occurred in aerial photography and photogrammetry with the start of World War II were as follows:

a. The grouping together of three cameras in the so-called "Tri-Metrogon" system for taking photographs simultaneously from horizon to horizon. This method was largely developed by the U. S. Army for purposes of making aeronautical charts; and at one time it was the purpose of the Army to map the entire world, to which it had access, by this method.

b. The recognition by the Army of the necessity for better performance in photographic airplanes. The airplanes were not fast enough, they did not have ceiling enough, and they were defenseless in the face of antiaircraft and fighter attacks. Minton Kaye, Colonel, U. S. Army Air Force, fathered the development of the "Tri-Metrogon" system, and was responsible for the assignment of fast, high-performance, military aircraft for aerial photography. He created squadrons of P-38s, capable of flying fast enough to elude enemy fighters, and high enough to elude antiaircraft fire. Toward the end of the war cameras were being carried by V-17s, B-24s, and B-29s. Some of these photo ships carried a formidable array of variegated cameras.

At Wright Field in Dayton, Ohio, George W. Goddard, Colonel, U. S. Army, was making great progress in camera design. He had prepared 40-in. telephoto lenses and developed the 9-in. by 18-in. reconnaissance camera, which made great strides in color photography, in addition to being responsible for the development of the very effective "Sonne" strip camera. The Navy, under L. A. Pope, R. H. Quackenbush, Jr., and J. H. McElroy, Captains, U. S. Navy, had readied itself for the particular aspects of photography suitable to it purposes.

There was an extensive domestic mapping program consisting of topographic sheets of coastal areas and also a substantial program of aeronautical charts and mosaics covering hundreds of thousands of square miles of North and South America. Extensive areas in many foreign countries had already been mapped by one method or another prior to hostilities, and these maps from many sources were recompiled into standard military maps.

"Tri-Metrogon" aerial photography was the most economical way of obtaining fair information of vast territories. It has much to commend it for general utility, for small-scale maps, and for charts.

For more detailed purposes considerable high-altitude, vertical photography was also done, at altitudes as great as 35,000 ft above the ground. At these altitudes pictures were procured at scales ranging from 1:50,000 to 1:70,000, and these were compiled into mosaic maps and used for topographical maps and other purposes.

The "Sonne" strip camera, an outgrowth of the photo-finish camera of the race track, was one of the few really new developments of the war. This camera has a roll of film moving continously across the focal plane at the same angular rate the airplane is moving over the ground. In conjunction with

the camera is a view finder, with "travelers." These are moving marks, which the operator can control so that they move at the same rate of speed as the image traverses the view finder. By so controlling the speed of the travelers, the speed of the film is regulated to move properly. The picture is taken through a narrow slit, and by this combination sharp pictures are obtainable from very low altitudes in a fast-moving airplane. This picture may be one continuous strip several hundred feet long. The "Sonne" pictures were extremely useful in taking inventory of troop movements along roads, and in photographing beaches at close range before landings.

In war there are two very satisfactory ways of obtaining aerial photography. The first of these is to fly high and fast, to be out of the range of antiaircraft fire, and high enough so that the danger of being attacked by fighters is minimized. The second satisfactory method is to come in low and fast, at altitudes of only a few hundred feet—perhaps at 100 ft or 200 ft. It is in the latter type of photography that the "Sonne" camera proved so effective. No other camera, moving at 300 miles per hr or more, at an elevation of 100 ft, would obtain anything except a magnificent blur.

The engineering profession emerged from the war with all these old and new tools to choose from. The writer has many biases in favor of the systems he knows best and believes in most thoroughly. Many of the arguments in this paper are expressions of personal opinion on subjects that are controversial, and it is hoped that possibly other people qualified to discuss the subjects will take issue with them, thus provoking constructive discussion.

The "Tri-Metrogon" system has only a limited application to civil demands in the fields of either aerial photography or photogrammetry. It may be used advantageously where working weather is at a great premium; and therefore it is desirable to photograph as much terrain as possible in the rare instances when suitable weather is available. Thus, for example, in Colombia, South America, where something less than an average of 2 min (photographic) per day has been experienced, there is justification for using "Tri-Metrogon" photography, because in the 60 min per month that can be expected on an average, much more ground can be photographed by "Tri-Metrogon," even though this ground is not nearly as well photographed and as usefully photographed as it could be by vertical methods. The "Sonne" strip camera is finding commercial application in conjunction with new developments such as the airborne magnetometer. It is also being used for taking large-scale inventory maps of railways and highways.

Aerial photography and photogrammetry are presented to the engineer today in much the same garb which dressed them in 1941, but with a much better press. In other words, more people are favorably impressed with aerial photography and photogrammetry, and the engineering profession in general is ready now (1949) to give it full acceptance.

A review of the types of project undertaken today by aerial photography and photogrammetry is probably the best way to crystallize engineering thinking on how these tools may best be applied to the requirements of modern engineering. For example, aerial photography first became important in the southwest in about 1938, when the Soil Conservation Service, the Forest

Service, and the Bureau of Indian Affairs (United States Department of the Interior), between them, completed aerial maps and mosaics of practically the entire eastern half of the State of Arizona, and the western half of the State of New Mexico. About this same time the Soil Conservation Service mapped the "dust bowl" area, comprising nearly 70,000 sq miles in Colorado, Kansas, New Mexico, and Texas.

In its earliest days photogrammetry was used to map the workings around the Boulder Dam site in Arizona and Nevada (1930); and later, when the gates of the dam were closed and the reservoir started filling with water, a contour map of the entire area of the reservoir was made. This map of the empty reservoir was designed as a base from which studies could be made to determine the rate at which the reservoir was being filled with silt.

Other dam sites of the Colorado River system have been explored extensively by photogrammetric methods. This work has been sponsored by the Bureau of Reclamation (United States Department of Interior). A contour map was made, for example, of the Bridge Canyon Dam site (about 10 miles above the headwaters of Lake Mead), at a scale of 1 in. = 500 ft, with 10-ft contours.

A very extensive survey was completed along the San Juan River in 1947, extending from approximately the confluence of the San Juan with its parent stream, the Colorado, up to the four corners where the states of Colorado, Utah, Arizona, and New Mexico come together. The topographical map to be prepared of this proposed reservoir is at a scale of 1 in. = 1,000 ft, with 10-ft and 20-ft contours.

This San Juan project is perhaps one of the most difficult topographical surveys ever undertaken. In fact, topographical surveys of any part of the Colorado chasm are very difficult, because the canyons themselves are almost impassable, and access to the canyons for establishing control is extremely difficult. Control of the San Juan job was largely done by jeep, pack horse, and on foot.

For years the Salt River Valley Water Users' Association of Phoenix, Ariz., has approached many of its problems from the starting point of aerial photography. Many of the tributary streams of the Salt River Valley system have been mapped from that valley up toward their sources. The entire irrigated area was mapped many years ago and has been used continuously for a variety of purposes. These maps have proved valuable to the Salt River Valley Water Users' Association as a complete inventory of physical conditions on the ground with which it is so vitally concerned; and in several instances they have given valuable record information which was used in litigation to prove conditions as they actually existed.

Over the years, the Bureau of Reclamation has had many photographic maps made of the unruly Colorado River. Areas subject to flood were studied from photographs made early in the project history. The delta of the Colorado, lying partly in the United States and partly in Mexico, has been photographed at least twice (the first time in 1922) to study the vagaries of this menacing river, which in flood thrashes itself from old channels to new channels, and back

again. The aerial photographic maps showed this delta in minutest detail, so that every channel, both new and old, was apparent, and the work of planning the control of the river was thus greatly expedited. Attempts have been made to hack ground survey lines through this brush-choked delta region, a prohibitively tedious and expensive task, yielding, at best, a bleak skeleton of data compared to the full body of exquisite photographic detail.

The mining areas of the southwest have utilized photogrammetry extensively. Many of the mines have had vast tracts photographed for geological exploration, whereas others have had detailed contour maps made of their operating properties. A photogrammetric map of a mining area offers two general types of valuable information. In the first place, the contact prints can be viewed stereoscopically and mosaics can be assembled, which together give a continuity and detail for delineating geology unobtainable by any other method. Secondly, the contour map that may be compiled from the same photographs gives an accurate base map on which roads can be located and construction projects of all kinds designed.

A paper such as this can never escape some discussion of relative costs. In the past it has been possible to give only the most generalized kind of cost information on aerial surveys, and that situation still persists. The aerial survey is not of a sufficiently definite nature so that its cost can be given, such as the costs of commodities or of tons of steel, or of bags of cement. The question—How much will an aerial survey cost—is comparable to asking: How much does a bridge cost to build? The estimator may even be told how long a bridge was to be, but he would still have difficulty in estimating its cost without knowing a great many other facts. The problem of supplying information about the cost of an aerial survey, and a topographical map based on photogrammetric methods, is just as complex (see Table 1). The following factors all affect such cost:

1. The location of the area;
2. The size of the area;
3. The weather conditions;
4. The specifications of the final map;
5. The accessibility of the area from the standpoint of control;
6. The existence or nonexistence of usable control already in the area;
7. The character of terrain from the standpoint of control;
8. The type of vegetation, from the viewpoint of both interference with control operations and interference with the ultimate precision of the contour map;
9. The maximum differences in elevation, which are an index of the number of contours that are to be drawn and drafted;
10. The number of roads, dams, railroads, and buildings, to be shown on the map;
11. The possible need of a map made on spherical coordinates;
12. The form in which the map is to be delivered—as lithographed sheets, tracings, or other form;
13. The number of geographical names to be shown;

14. The extent of field editing required: How critical is the map from the standpoint of showing every small building, and how serious would it be if a haystack were mistaken for a building and shown on the map?;
15. The season of the year the work must be done;
16. The time to be allowed the contractor; and
17. The required scale, the contour interval, and the accuracy.

Some data relating to costs of contour maps for highways, and for city planning and land subdivision, are offered in Table 1. In addition, the following general data are offered as applying to reservoir mapping:

Item	Description	Unit cost
1	Thirty-seven reservoirs, some at scales of 1:1,200 with 5-ft contours and some at scales of 1:12,000 with 10-ft contours; dollars per reservoir	$4,000
2	Two reservoirs at 1:6,000 with 10-ft contours; dollars per reservoir	6,500
3	Sundry reservoirs aggregating 75 sq miles, some at scales of 1:10,000 and some at scales of 1:24,000—all at 5-ft contours; dollars per acre	1.02
4	Sundry reservoirs, one group at 5-ft contours aggregating 40 sq miles and another group at 20-ft contours aggregating 24 sq miles—all at scales of 1:6,000; dollars per acre	0.90
5	Sundry reservoirs aggregating 31 sq miles at a scale of 1:4,800 with 5-ft contours; dollars per acre	0.98

TABLE 1.—Cost of Contour Maps by Photogrammetry, at 1947 Prices

Item	Contour interval (ft)	(*a*) Highways				(*b*) City Planning and Subdivision		
		Scale 1 in. equals (ft)	Dimensions, in miles[a]	Dollars per acre	Dollars per sq mile[a]	Scale 1 in. equals (ft)	Area, in acres	Dollars per acre
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1	5	100	0.38 by 20	3.34	(870)[b]	100	5,760	1.00
2	5	100	....	6.00	....	100	400	7.25
3	5	200	(300 ft wide)	15.40	(570)[b]	100	1,500	4.76
4	5	200	1 by 8	1.38	885	200	9,600	1.72
5	5	200	1.14 by 20.5	0.98	629	200	450	4.90
6	5	200	....	0.85	545	....	....	....
7	5	200	(6 sq miles)	1.82	....	....	....	....
8	5	200	(49 sq miles)	1.02	....	....	....	....
9	10	....	....	....	....	200	9,600	0.77
10	10	....	....	....	....	300	1,400	0.93
11	20	....	....	....	....	....	....	....

[a] Except quantities in parentheses. [b] Dollars per linear mile.

In considering the cost of topographic maps by photogrammetric methods, one very essential differentiation must be kept in mind between this method of map making and the older and more conventional methods. With the photogrammetric method, the horizontal scale is usually not the determining

factor in cost; the vertical accuracy is the all-important element. Flying height is largely a question of vertical accuracy of the contours. Various methods of photogrammetry permit flying the airplane at certain altitudes for certain results, and therein lies the efficiency of the method. The cost may vary many-fold depending on the method and the experience at the disposal of the map maker.

Contour maps made by photogrammetric methods within a certain range can be enlarged and reduced to almost any horizontal scale; but the minute that 5-ft contours are specified, and it is required that 90% of the points checked on the contours shall be accurate to half a contour interval, then it is immediately required that flying altitude (with the Fairchild system, for example) be 2,500 times 2.5 ft, or 6,250 ft aboveground. Thus, in all large-scale contour maps made by photogrammetric methods, the contour interval is a very important factor. For example, if a 5-ft contour is required, the map could be produced very efficiently at 1 in. = 200 ft, or 1 in. = 400 ft. Any one of several other scales could be selected without affecting the cost very greatly. However, if the contour interval were changed from 5 ft to 10 ft, although the scale remained the same, many phases of the operation would be reduced to one fourth—there would be only one fourth the number of control points to establish, and only one fourth the number of pictures from which the map had to be drawn. Also, there would be one half the amount of flying. Thus, the contour interval and vertical accuracy are all-important factors in the cost of topographical mapping by photogrammetric methods.

This same thought can be expressed in another manner. For example, if a 10-ft contour map is being prepared at a horizontal scale of 1 in. = 1,000 ft, and the specification requires that 90% of the points tested on the contours shall be within a half interval of their true elevation, the resultant map will cost nearly twice as much as if only 75% of the points tested has to be within a half contour interval of the true elevation. The flight can be made twice as high if only 75% of the points must check in vertical accuracy.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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REPORTS

RATES AND RATE STRUCTURES IN WATER AND SEWAGE WORKS

PROGRESS REPORT OF THE CHAIRMAN, COMMITTEE OF THE SANITARY ENGINEERING DIVISION ON FUNDAMENTAL CONSIDERATIONS IN RATES AND RATE STRUCTURES FOR WATER AND SEWAGE WORKS

This committee of the Sanitary Engineering Division is cooperating with similar committees or representatives of a number of other societies and associations, so that no statements can be made which would tend to commit any of the group prematurely. Much of interest, however, is of record which can be reported.

The committee began to take form at a meeting of the Municipal Law Section of the American Bar Association at its annual convention in Cleveland, Ohio, in September, 1947. For a number of years the chairman has expressed his views as to the unfairness and inadequacy of a considerable number of rate structures; and he was therefore invited to present a paper on this matter to the Municipal Law Section.[1] At this meeting (September, 1947) a committee of the Municipal Law Section was suggested and later approved to cooperate with a similar committee of the ASCE and other interested organizations to study and report on fundamental considerations in rates and rate structures. This action was reported to the Excutive Committee of the Sanitary Engineering Division, ASCE, and to the Board of Direction, ASCE, who approved a similar committee. Later in 1948 the following members of the ASCE were appointed:

E. H. Aldrich, Chief Engineer, Water Works Service Company, New York, N. Y.
T. J. Blair, Jr., President, West Virginia Water Service Company, Charleston, W. Va.
C. J. Derrick, Commissioner, Board of Public Works, Los Angeles, Calif.
F. S. Friel, Consulting Engineer, Philadelphia, Pa.

NOTE.—Please forward all comments on this report directly to Chairman Samuel A. Greeley, 220 South State Street, Chicago 4, Ill. Progress reports are published in *Proceedings* only.

[1] *The Daily Bond Buyer*, December 1, 1947.

G. J. Schroepfer, Professor, University of Minnesota, Minneapolis, Minn.
G. P. Steinmetz, Chief Engineer, Public Service Commission of Wisconsin, Madison, Wis.
Samuel A. Greeley, Engineer, Chicago, Ill., *Chairman*

The organization of the group and its coordination with the committee of the American Bar Association proceeded during the summer, and other organizations were included so that at present the following six are represented in the joint study:

American Bar Association
American Public Works Association
American Society of Civil Engineers
American Water Works Association
Federation of Sewage Works Associations
Municipal Finance Officers Association

Representatives of these organizations, except those of the American Water Works Association, met in Chicago, Ill., on October 15 and 16, 1948, at which time the scope of the problem was thoroughly discussed, subcommittees were named, and topics were assigned for study. The second meeting of the group is planned to be held in Chicago on April 15 and 16, 1949.

At this meeting (October, 1948) it was considered that the scope of the study should be limited to fundamental considerations only and should include financing as related to rates and rate structures, both matters being concerned with private and public utilities. An initial statement of objectives was tentatively agreed to as follows:

(*a*) Determination of whether or not, and how, a rate structure can be computed that will fairly allocate the contributions of persons and property to the necessary annual revenue on the basis of benefits and costs.

(*b*) Consideration of what constitutes the total revenue requirement to which the rate structure applies, and its effect on the rate structure.

(*c*) Review of the constitutional and statutory considerations and influences that affect rates and rate structures and related financing, and interpretations of the courts thereon.

(*d*) Preparation of a report useful to those undertaking the instituting and administering of, or having jurisdiction over, water and sewage works rates and rate structures and related financing.

It is pertinent to explain briefly the early statements and considerations that have prompted the organization of this joint undertaking and have resulted in these objectives. There is so much of record in technical journals regarding the unfairness and diversity of many water and sewage rate structures that no attempt will be made to describe this matter. One writer has stated that rate structures generally appear to have been determined on almost every basis except the proper one. Since the historic decisions of the United States Supreme Court designated as National Water Works versus Kansas City in 1894 and Smyth versus Ames in 1898, more than seventy important court

decisions can be cited relating to utility values and rates. These decisions seem to have been directed largely to the computation of a fair annual return and revenue and not to the determination of a fair payment by each recipient of a service or user of a commodity. In commenting on this condition in his address before the Municipal Law Section of the American Bar Association, the chairman stated:

> "* * * as to fairness, the rate structure and the resulting revenue should be fair not only to the utility but also to each individual on whom the utility depends for its income."

Current attitudes and practices as regards rate structures are reflected by a quotation from a book by Anson Marston, Past-President and Hon. M. ASCE, and the late T. R. Agg, M. ASCE, in which are presented five principles of public utility valuation, the fifth of which reads as follows:[2]

> "* * * The legal principle which most often applies in determining fair public-utility rates is that the utility owners are entitled to a fair rate of return upon the present fair value of the property used and useful in rendering the public service; but this principle is subservient to the ruling principle that the public is entitled to demand that no more shall be exacted from it for the services of a public utility than the services rendered are reasonably worth."

It has seemed to the chairman that this principle should be extended to include the statement that no more shall be exacted from each individual or unit user of the service or commodity than his or its fair share of the cost of providing such service or commodity.

The purpose of the work of this committee is, jointly with others, to attempt to state the fundamental considerations on which the computation of fair rate structures may be based for a given set of conditions. Basic topics have been stated, considered, and assigned for study and report to various members of the joint group, such as:

(*a*) Do water and sewage works offer for sale a commodity or a service, or both?

(*b*) What is the legal status of rates and rate structures in accordance with court and commission rulings?

(*c*) What effect do the physical characteristics of a municipal corporation and its environment have on the form of rate structures?

(*d*) As regards revenue financing and rate structures, how should they be related to other methods of financing, such as general obligation bonds, frontage assessments, benefit assessments, and current taxes?

Topics such as these, together with definitions of terms, bibliographies, and the like, are being prepared for discussion at the proposed meeting of the joint group in Chicago on April 15 and 16, 1949.

The topic assigned to the committee (fundamental considerations in rates and rate structures for water and sewage works) seems to have aroused a

[2] "Engineering Valuation," by Anson Marston and T. R. Agg, McGraw-Hill Book Co., Inc., New York and London, 1936, p. 172.

considerable interest in those who are concerned with related legal, financing, and engineering problems. The organizations that have joined with the ASCE committee have made an enthusiastic beginning. It seems likely that 2 years will be required to complete the study and the final report. In the meantime it is hoped to enlist the interest, cooperation, and support of ASCE members.

Respectfully submitted,

Samuel A. Greeley, *Chairman*

Committee of the Sanitary Engineering Division on Fundamental Considerations in Rates and Rate Structures for Water and Sewage Works

January 20, 1949

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

EXPERIENCES WITH PREDETERMINING PILE LENGTHS

Discussion

BY WILLIAM W. MOORE

WILLIAM W. MOORE,[31] M. ASCE.—The comments and suggestions offered by the several discussers and informal suggestions received from interested members of the engineering profession are sincerely appreciated.

The comments offered by Mr. Earl are of particular interest and value because of his experiences both with the method suggested by the writer and other more conventional methods of controlling the installation of pile foundations. Mr. Earl's comment that the method suggested enables taking into account the effect of settlement in upper layers of soil, such as filled-in areas, is believed to be particularly important because the need for pile foundations most frequently arises in such areas. Most other methods for evaluating the behavior of pile foundations offer no opportunity for even approximating the influence of upper subsiding strata.

As suggested by Mr. Earl, the obtaining of accurate information on the subsurface conditions by borings is of the greatest importance. When a sufficient number of borings is available, the important features of the foundation soil structure usually become apparent; however, minor variations in the strengths of individual strata at various locations on the site may be such that it is futile to attempt extreme precision in determining the lengths of piles required. The problem, then, becomes one of determining a safe and practicable length of piling for use on a site or a particular part of a site. The important decisions are to be certain that the lengths selected are adequate but not excessive and that these pile lengths can be installed practicably with available construction methods and equipment.

The comment by Mr. Feld that the basic concepts of the method of analysis presented by the writer have been previously suggested by various investigators only suggests that sufficient study must be directed to improve modern knowledge to a degree which will restrict the uncertainties and errors so that prac-

NOTE.—This paper by William W. Moore was published in November, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1948, by A. W. Earl; June, 1948, by Hamilton Gray, Jacob Feld, and George F. Sowers; and February, 1949, by H. Cambefort.

[31] Partner, Dames & Moore, San Francisco, Calif.

ticable results can be assured. The comments of Mr. Sowers are most interesting in describing the development by entirely separate investigators of a method of analysis which appears to be parallel to that suggested by the writer. Mr. Feld's comparison of the strength of a laminated beam consisting of nonequal planks laid flat on each other to the strength of a pile penetrating several soil layers is applicable; if the planks were to be imagined as having the property of continued yielding without severely reduced strength, it would be true that the ultimate strength of the beam would be approximately indicated by arithmetic summation of the strengths of the several planks. It may be suggested that, where the stiffnesses or strengths of the several soil strata do not differ by a wide order of magnitude, the strengths of all the strata may be almost fully mobilized before any actual failure or substantial slippage of the pile relative to the soil can occur. Within the range of working loads, it is quite possible that the stiffer strata may carry much more than their estimated proportion of the total imposed load. However, if no failure can occur without developing resistance in each of the strata, it would appear that the method suggested offers an opportunity for safely evaluating what may be termed the yield point bearing capacity for the pile. The computations can be readily modified to include only the supporting value of firm strata, in cases where certain soil strata may be too weak or compressible to contribute to the safe supporting capacity of the piles.

The writer has fully recognized, as suggested by Messrs. Feld and Sowers, that precise determination of the lateral pressure of the soil against the side of a pile is a quite complex and indeterminate problem. However, he is unable to accept the thesis that the lateral pressure against the side of a driven pile can be zero, or even that it can be less than the vertical weight of overburden above any given depth. Such conditions of low lateral pressure have been observed, as noted by Mr. Feld, in the walls of drilled or excavated shafts where the arching of the material can permit internal soil stresses to develop which result in very low or zero lateral pressures against the casing or bracing within the shaft. Since these circumstances appear to be quite different from those surrounding a pile forced into the soil with accompanying lateral displacement and compression of the surrounding material, it appears reasonable to presume that the lateral pressure on the side of a pile will be greater than the weight of adjacent overburden; and, furthermore, that the intensity of the pressure will be related to the shearing resistance and probably also to the compressibility of the material displaced during the driving of the pile. The equation used by the writer for evaluating the lateral pressures on the surface of the pile (Eq. 1) is noted to be largely empirical and has been reached through analyses of the behavior of piles during static loading tests. The equation suggested is not intended to provide a rigorous theoretical solution for the potential lateral pressures. However, it has generally been found that, if the increments of lateral pressures in excess of the weight of overburden are assumed to lie within a range on the order of three to five times the shearing strengths of the materials penetrated, the resulting calculations have been reasonably consistent with the observed results of static loading tests on full-sized piling. The relatively higher lateral pressures are usually indicated for quite sandy or gravelly soils.

Some studies made in 1948 and 1949 by the writer and his associates have involved the installation of strain gages in full-sized piling under test conditions, and it is believed that analyses of these observations will throw some light on the distribution of frictional forces along the length of the piles[32] and in turn may indicate the intensities of the lateral pressures developed. In this connection, it may be noted that a report[33] by Armand Mayer, M. ASCE, published in 1947 describes a concept of analysis almost identical with that suggested by the writer, except that Mr. Mayer's analysis results in a substantially higher evaluation of the end bearing factor when piles are driven into granular deposits. Recent experiences have indicated that the computations suggested by Mr. Mayer may result in an improved evaluation of the field behavior of piling in granular deposits, although some questions still remain as to the permanent dependability of very high end bearing resistances and the behavior of piles so supported when subjected to vibrating loads.

During recent years, the writer and his associates have developed, in collaboration with the late Ralph Byrne, Assoc. M. ASCE, the following equation as perhaps offering a somewhat improved evaluation of the end resistance of piling in sand and gravel deposits:

$$P = \frac{\pi C + p\,[1 + (\pi/2 + \phi)\tan\phi]}{1 - (\pi/2 - \phi)\tan\phi} \qquad (18)$$

in which P is the limiting bearing pressure; p is the surcharge of weight of overburden adjacent to footing; ϕ is the angle of internal friction of soil; and C is the cohesive shearing strength of soil.

The numerical values of potential skin frictional support for piles in sand, indicated by the formula presented by Mr. Cambefort (Eq. 15), result in quite high values which do not conform with the field loading test data available to the writer. Although the evaluations of frictional resistance and end bearing indicated by Eqs. 5, 6, and 7 suggested by the writer are known to result in rather low values for some sandy soils, it is his opinion that the possible magnitude of safe increases of these values depends on factors which at present are not completely understood. Some of these interrelated factors are the density and the compressibility of the deposits penetrated, the volume of the pile, and the weight and the energy of the driving mechanism used. It is the writer's belief, therefore, that the values should be increased above those suggested in the paper only where quite complete local data are available, including loading tests made on the specific site with pile sizes and driving conditions comparable to those of the ultimate structure.

As suggested by Professor Gray, the use of static field loading tests is often a most valuable aid in determining safe and adequate piling installations. Although the writer has used the proposed method of computing capacities on many projects without the assistance of static field loading tests, it is preferable to have such loading test data whenever the economic factors involved in the project will permit. The experience of the writer has indicated, however, that

[32] "Electrical Resistance Strain Gages for Determining the Transfer of Load from Driven Piling to Soil," by L. LeRoy Crandall, *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, June 21 to 30, 1948, Vol. IV; p. 122.

[33] "Some Recent Tests on the Bearing Capacity of Piles," by A. Mayer, *Pamphlet No. 34*, Series 1, Technical Inst. of Constr. and Public Works, Paris, March 31, 1947.

field bearing tests on piling or driving records of test piling are frequently of little value and perhaps may even be seriously misleading unless knowledge of the character of the soils into which the piles are driven is reasonably complete. It has been found that the expense involved in making the soil tests and analyses of pile lengths is not usually a serious burden, since this cost seldom exceeds approximately one third to one half of the cost of making borings, taking samples, and studying purely visual descriptions of the subsurface materials.

The pile loading test described by Mr. Cambefort illustrates a very desirable type of test in which the skin friction on the lateral surfaces of the pile can be distinguished quantitatively from the end resistance on the bottom of the pile. Data of this character are certainly of great assistance in understanding the factors influencing the supporting capacity of piling. The circumstances described wherein the test pile penetrated alternately firm and very soft strata illustrate the need for evaluating the compressibility of the several strata, either purely by engineering judgment or preferably by some definite test data, in order to estimate the dependable supporting capacity of each of the soil layers. It has been the writer's practice to reach a decision as to which strata will be depended on for the primary support of the piling by consideration of the texture, firmness, moisture content, density, and sometimes consolidation characteristics. In numerous cases, it has been decided that certain strata which overlie weak and very compressible layers will be either completely neglected in computing the permanent potential support or considered as possibly imposing downward frictional forces as a result of expected future subsidence of these upper strata.

The discussion by Professor Gray concerning the disturbance of the soils immediately surrounding a driven pile and the possible variations in soil strength with time is quite pertinent and correct. The suggested thesis that the strength of the undisturbed soil may be regained after a time appears to be substantiated by the investigations reported in 1948 by A. E. Cummings, M. ASCE, G. O. Kerkhoff, and R. B. Peck,[34] Assoc. M. ASCE. In view of these studies, as well as the experiences of the writer and his associates, it is believed that the bearing capacities determined by the method of analysis presented are generally conservative, and under certain conditions may be appreciably below the ultimate bearing capacity of the piling.

Although the examples cited by the writer did not include cases where any large settlements had developed, it seems unduly optimistic, as mentioned by Professor Gray, to presume that a clay firm enough to carry the design load on one pile without slipping would in all cases carry the imposed load without appreciable volume consolidation. The settlement resulting from volume consolidation would depend largely on the intensity of stress and the area loaded by the pile groups. In instances where large pile groups may impose fairly heavy loads over an appreciably large area, it is believed that studies of the possible volume consolidation of the soils beneath the pile tips are essential.

Although the behavior of pile groups was not included within the scope of the paper, the behavior of such groups may be, and has been, evaluated within

[34] "Effect of Driving Piles Into Soft Clay," by A. E. Cummings, G. O. Kerkhoff, and R. B. Peck, *Proceedings*, ASCE, December, 1948, p. 1553.

practicable ranges of accuracy by considering the stresses in the body of soil bounding the pile group and directly beneath the pile group. For example, the sum of the potential shearing resistance around the periphery of the pile group, plus the bearing resistance of the material directly beneath the pile group, may be evaluated as if the group were a single large foundation, such as a caisson. Evaluation of the probable factor of safety against failure of the pile group as a whole may be made in this fashion, as well as evaluation of the probable settlements due to compression of the soil materials on which the piles themselves are dependent for support.

Professor Gray's suggestion of the need for definition of the yield point shearing strength may perhaps be answered by illustrating the results of typical shear tests of two materials with the following characteristics:

Description	Sample 1	Sample 2
Material	Clay	Medium sand, with some small gravel
Dry density, in pounds per cubic foot	91.4	104.4
Moisture content, in percentages of dry weight	38.9	21.6
Surcharge, in pounds per square foot	1,650	3,100

These are presented graphically in Fig. 26. In general, the shear tests used by the writer and his associates have been made by applying increments of loading at 1-min intervals of time in a direct ring-type shearing apparatus and measuring the resulting deflections.[35] The stress-strain graph is examined and the yield point selected as the load above which rapidly disproportionate increments of deflection are noted for equal increments of loading. There is evidence that other time rates of testing (perhaps in some cases very much more rapidly, and in some cases very much more slowly, than the tests generally made previously) may offer a method of approximating the applicable field conditions more closely and thereby enabling better evaluations of the behavior of piling on a particular site. Mr. Feld's comment that the frictional resistance for various rates of movement may be important in the resistance of piling is most interesting and pertinent. It is believed that the use of an extremely wide range of rates of deflection for testing may be of assistance in evaluating the ratio of the static bearing capacity to the dynamic driving resistance; also, such data may assist in estimating the dependable static supporting capacity of jacked-in piles in relation to their resistance while being jacked into place.

In paragraph 4 of Mr. Feld's discussion, it is noted that the average friction along the surface of the pile cannot be based on a coefficient of friction greater than that of the soil itself. The numerical values of soil properties used in the sample calculation presented in Fig. 2 of the paper indicate that the shearing resistance of the soil includes a cohesive resistance of 450 lb per sq ft and an angle of internal friction of 14°. The shearing resistance for the lower ranges of surcharge pressure, when both the cohesive strength and internal friction are included, will exceed the frictional resistance between the pile and the soil based on a coefficient of friction of 16°.

[35] "Practical Shear Tests for Foundation Design," by Trent R. Dames, *Civil Engineering*, December 1940, p. 783.

It has generally been the writer's experience, as suggested by Professor Sowers, that clayey soils develop an adhesion to the pile so that the actual failure takes place in the soil close to the pile surface rather than with a strictly frictional movement between the pile surface and the soil. Since it is nearly always difficult to classify materials as purely cohesive or purely granular, and since a very large number of soil deposits combine some of the properties of the cohesive, and some of the properties of the granular, materials, the writer pre-

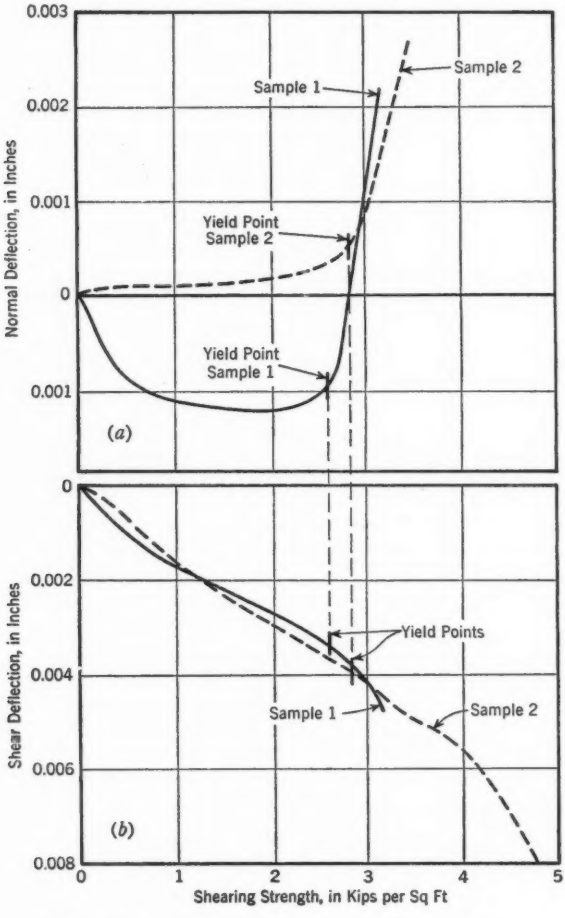

FIG. 26.—STRESS-STRAIN CURVES FROM SHEAR TESTS

fers to use test methods that include the influence of both the cohesive and the frictional factors. With these methods, the test results will place an appropriate relative importance on the different properties of cohesion and internal friction for a specific soil condition.

Professor Sowers' experiences with the use of unconfined compression tests do not appear to be fully confirmed by the writer's experiences. In many

clayey soils encountered during investigations, it has been found that neglecting the influence of confining pressures during the tests may result in serious errors and sometimes excessively low indications of strengths for the available soil deposits. Frequently, it is difficult to distinguish, by visual examination, between purely cohesive clays and those containing significant proportions of very fine granular particles. It is the writer's belief, therefore, that, until experience has fully confirmed the reliability of the unconfined compression test for a particular local soil condition, a considerable number of tests should be made which evaluate the influence of confining pressures on the strengths of the soils.

The studies made by Professor Sowers using the results of remolded shear tests are especially interesting. The writer believes that such remolded tests should certainly be made when clays having a characteristically severe loss in strength on remolding are encountered. However, the long-time supporting capacities of piling driven into such clays may be less seriously affected than would at first be indicated by considerations of the remolded strengths. Further research, and perhaps pile loading tests made some time after driving, may produce valuable information on this problem. The stress conditions suggested in Professor Gray's discussion and the data presented by Messrs. Cummings, Kerkhoff, and Peck[34] are indications of the direction which these investigations may take.

As suggested by Professor Sowers, the use of jetting to sink piling into sand and gravel deposits interposes an additional and frequently highly indeterminate factor—probably influencing both the lateral pressures and, in some instances, the end resistances of piling placed in sand and gravel deposits.

As Professor Gray suggests, the results of computations utilizing the suggested method should be accepted cautiously and with much study of local experience records. In many instances, a careful study of local experience records together with analyses and perhaps field loading tests will indicate how appreciable economies in foundation construction may be realized with full safety for the structures involved. Also, the type of studies described may enable the design of foundations capable of much more desirable performance than would be achieved with more conventional design methods, and frequently at an equal or lower construction cost. However, where the lengths or loadings of piling indicated by the method of analyses suggested result in marked differences from the current local practice, thoroughly adequate investigations and studies should be made to explain fully the circumstances and the reasons for these differences in order that the satisfactory behavior of any new installation may be assured. As commented by several of the discussers, observation of high driving resistance is not reliable proof that the driven piles are capable of supporting the design loads. On the other hand, a relatively low driving resistance under certain conditions may not in any degree warrant a conclusion that the piles are inadequate to support the planned loads. In many cases, large increases in pile lengths required to realize conventional dynamic driving resistances constitute a completely unnecessary and severe economic waste.

Mr. Feld's comment that the *Engineering News* formula $\left(\text{which was used in the paper in the form of } P = \frac{2\,w\,H}{s + 0.1}\right)$ is not applicable for heavy hammers, such as a Vulcan No. 1 and Vulcan No. 0, makes it the more imperative that the profession develop a practical and reliable method of evaluating the behavior of piling because the present-day trend is increasingly toward the use of heavier driving equipment and higher design loads per pile.

Summary.—The consensus of the discussers' contributions indicates the basic concepts of the method of analysis presented have been suggested at various times in the past by leading investigators and will eventually be developed by the profession to a point where such a method will be of substantial assistance in the design of safe and adequate pile foundations. The use of exploratory borings and of adequate sampling of the soils is of primary importance in applying the method of analysis suggested and in understanding the subsurface conditions and the behavior of the foundation soils on any site. The use of pile driving tests and of pile loading tests may be of great assistance in evaluating construction problems and in confirming analyses of the supporting capacities of individual piling. However, such data can be utilized with assurance of reliability only when there is adequate knowledge of the structure and the properties of the subsurface soil materials. Consideration of the stresses produced in the foundation soils surrounding and below clusters of piling is essential when structures involving any very heavy loads are to be supported, or when large pile groups are to be erected, on sites where soils of appreciable compressibility extend to depths below the pile tips.

The results of the method of analysis suggested should be compared carefully and studied in relation to local experience and practice; and, where apparent differences between the results of the analyses and local practice appear, the reasons for such differences should be analyzed and fully explained. After studying the soil properties and perhaps results of pile loading tests for any specific project, it will be feasible and desirable to establish some criteria for driving resistance (not necessarily related to any dynamic formula) as a method of field control of the pile driving during the actual construction so that any important and unexpected differences in soil conditions would be indicated by the pile driving and appropriate measures could be taken to assure adequate and satisfactory support.

Additional study and research concerning the intensities of lateral pressures created when driving piling into various soil types are needed; and, similarly, laboratory research and field measurement of the end resistances, particularly in granular soils, will doubtless result in improvement and revision of the detailed methods of calculation. Future field loading tests and research programs should include measurements of the stress transmitted from the pile to the soil at various elevations and determinations of lateral pressures on the sides of the piles together with studies of the changes in these effects over periods extending for many months after the piling is driven.

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DISCUSSIONS

ADJUSTMENT OF THE "SHORAN" TRIANGULATION

Discussion

BY CHUNG-JUI CHU

CHUNG-JUI CHU.[9]—Several valuable ideas have been added to the subject of the paper by Mr. Sheldon. However, it should be pointed out that the solution of equations of the type of Eq. 6 will give the angular values of the corrections expressed in radians to the approximate positions, not the linear values as mentioned by Mr. Sheldon. It is a simple matter to express the corrections in seconds by multiplying the right-hand member of Eq. 6 by a constant. Furthermore, the writer does not agree with Mr. Sheldon in the statement that the method is suitable primarily for computation of geodetic positions. To use the direct method of computing the geodetic positions the computer must first determine the adjusted values of the measured distances. Such a method is similar to the conventional method, but is very laborious. On the contrary, by the writer's method the geodetic positions are obtained by adding the corrections to the approximate positions, which may be just astronomic positions.

Captain Hemple has given more valuable information on this new technique of measuring the distances and has stated that in the near future the "shoran" method may be used for first order triangulation.

The shoran method should find wider application in the field of surveying. It may yet be applied to the positions of the airplane in aerial surveying, the locations of the traverse stations, the finding of a suitable world-wide geodetic datum, and the determination of the dimensions of the spheroid which is taken to represent the earth. When that time comes, geodetic engineers will have seen great changes in the field operations and the office computations of surveying.

In closing, the writer wishes to express his gratitude to Mr. Sheldon and Captain Hemple for their contributions to the paper.

NOTE.—This paper by Chung-jui Chu was published in September, 1948, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1949, by Robert C. Sheldon, and H. W. Hemple.

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DISCUSSIONS

STABILITY OF THIN-WALLED COMPRESSION MEMBERS

Discussion

BY H. DUDLEY WIMER, JR.

H. DUDLEY WIMER, JR.[18]—The discussion by Professor Winter was most welcome and informative. Several points were emphasized which had not been completely covered in the paper.

As was remarked by Professor Winter, it is very gratifying to note the agreement between his investigation and that described in the paper. Eq. 16 is admittedly a refinement and of more general application to different materials.

However, it should be noted that Professor Winter's tests were performed an open, channel-type sections where torsional instability was at least a contributing factor to failure. Undoubtedly, had closed sections been included, their failing loads would have been higher, and the indicated effective widths greater. Consideration of this factor would tend to bring the two investigations into even closer agreement.

In the opinion of the writer the most unique features of the method presented in the paper are:

(1) The separation of sheet metal compression members into flat and curved elements, and recognition of the fact that their behaviors are fundamentally different; and

(2) The practical simplicity of its use, including the tabulating of values for flats and bends which may then be combined to suit a large variety of sections.

The work has been sponsored for publication as a project of the Committee of the Structural Division, ASCE, on Design in Lightweight Structural Alloys.

NOTE.—This paper by H. Dudley Wimer, Jr., was published in May, 1948, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: November, 1948, by Boris Bresler, and George Winter.

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DISCUSSIONS

LATERAL EARTH PRESSURES ON FLEXIBLE RETAINING WALLS

A SYMPOSIUM

Discussion

BY GREGORY P. TSCHEBOTARIOFF

GREGORY P. TSCHEBOTARIOFF,[63] M. ASCE.—The writer expresses his appreciation to all discussers, but wishes to give an evaluation of such criticisms as have been made.

Professor Roberts offers what he terms a "most serious criticism" of the fact that descriptions of experimental techniques were not given more space in the writer's paper. On the other hand, the original draft of the writer's paper was criticized for the opposite reason on the grounds that the vast majority of the members of the Society are more interested in actual findings than in laboratory techniques. Although he has endeavored to keep a reasonable balance within the space limitations imposed, the writer concurs with the latter point of view. Detailed descriptions of such techniques should be relegated to specialized publications. This was done to the extent possible at the time[36,64] and has been developed since.[65]

Professor Roberts writes: "* * * the Carlson pressure cells used for measurement of end reactions are stated to have caused trouble by clogging." No such statement is made in the paper. A clear differentiation is made in the paper between the two types of cells—Carlson pore pressure cells (which measured the excess pressures of the water filling the voids of the clay and the porous stones of which got clogged) and the two Carlson cells used for the measurement of vertical reaction pressures of the metal bulkhead. A perfect

NOTE.—This Symposium was published in January, 1948, *Proceedings*. Discussion on this Symposium has appeared in *Proceedings*, as follows: June, 1948, by Ralph B. Peck, Howard C. Roberts, Armand Mayer, S. Packshaw and J. Owen Lake, and Jacob Feld; September, 1948, by E. De Beer; and October, 1948, by D. P. Krynine.

[63] Associate Prof. of Civ. Eng., Princeton Univ., Princeton, N. J.

[64] "Use of Electric Resistivity Strain Gages Over Long Periods of Time," by Gregory P. Tschebotarioff, *Proceedings*, Soc. for Experimental Stress Analysis, Vol. III, 1946, No. 2, pp. 47–52.

[36] "A Method of Effecting SR-4 Strain Gage Operation Under Water," by Edwin L. Kimble, *ibid.*, pp. 53–54.

[65] "Determination from Bending Strain Measurements of the Distribution of Lateral Earth Pressures Against Model Flexible Bulkheads," by Gregory P. Tschebotarioff, *Géotechnique*, London, Vol. 1, No. 2, 1948, pp. 98–111.

understanding of this differentiation will serve to clarify many of the questions raised by Professor Roberts.

In commenting on Part I of the paper, Professor Roberts states:

> "The wire type resistance strain gages (apparently) used are subject to many sources of error, but most of these errors are systematic—not random. The scattering of points suggests mechanical difficulties * * * ."

The term "electric resistivity strain gages" was used repeatedly in the text of the paper and the gages were even identified by their trade-mark as "SR-4" gages. Reference was made[64] to an extensive description of the type, mounting, insulation, and other details pertaining to these gages. Thus Professor Roberts is correct when he states that the gages were of the wire type; and he need not have qualified his statement with a parenthetical "apparently." Furthermore, the writer disagrees with the assertion that most errors of such gages are systematic and not random. The reverse is true, as can be seen from Fig. 33 (which shows the erratic deviations from the initial readings of gages mounted on a steel plate which was described in the paper as being "unstressed"). None of the mechanical difficulties suggested by Professor Roberts were thus possible during that test; nor were they possible during any of the tests with model bulkheads. Systematic studies and observations by the writer's laboratory since 1941[64] indicate that even after elimination of instrument errors there occur some erratic variations when readings are taken over long periods of time, because of uncontrollable minute volume changes of the cement by which the SR-4 gages are attached. The erratic nature of these slight variations, when understood, permits their control and elimination by the use of a sufficiently large number of gages. For that reason 105 separately measured gage pairs were used during the third series of tests.

Professor Peck admits that the results of the tests performed with backfilled sand dikes "are of interest and of value"; and, on the other hand, he accepts the Princeton testing techniques as giving "highly important" information in that case. Nevertheless, he claims that exactly the same testing techniques give "disappointing" and "misleading" information in the case of backfills composed entirely of sands. The only apparent difference between these cases lies in the fact that sand-dike tests had never been performed before; and they had never been dealt with in textbooks. Consequently, their results were not in conflict with any previous conventional ideas on the matter. Therefore, prior to giving further and conclusive evidence proving the previously unsurpassed accuracy of the entirely new testing methods in the Princeton tests, it appears advisable to glance at the meager factual and experimental basis for these conventional ideas, especially in so far as the so-called "arching" of sands behind flexible bulkheads is concerned. Supplementary reference is made to appropriate passages of the writer's final report[66] on this entire test program.[66a]

Discussions related to this topic arose in June, 1948, at the Second International Conference on Soil Mechanics and Foundation Engineering at Rotter-

[66] "Final Report to the Bureau of Yards and Docks, Department of the Navy, on Large Scale Earth Pressure Tests with Model Flexible Bulkheads," by Gregory P. Tschebotarioff, Princeton Univ., Princeton, N. J., January 31, 1949.

[66a] A copy of this report has been filed in the Engineering Societies Library (29 W. 39th St., New York 18, N. Y.), and the writer will be glad to send at nominal cost a copy to members of the Society who address their requests to him (Eng. Bldg., Princeton Univ., Princeton, N. J.).

dam, Holland (The Rotterdam Conference) concerning papers by the writer and his associates.[67,68,69] These discussions[70,71] revealed the interesting fact that at least one of the authors of the Danish rules[26]—A. E. Bretting—appeared to consider that arching in a vertical direction behind a deflected sheet pile bulkhead must be present since it is known to be effective when the pressure area forms a complete circle around a shaft or a tunnel or when forces act in a horizontal direction behind sheeting of a vertical cut rigidly braced at its top at the start of the excavation. The writer pointed out[71] that the stability conditions of these types of arching are completely different, that they are in no way directly comparable, and that they should not be confused with each other. Some of this confusion may perhaps be the result of improper interpretation of statements such as the one made by Karl Terzaghi, Hon. M. ASCE, who wrote[72] that he was impressed with "the infallible and universal efficacy of arching." The Symposium authors repeatedly emphasized that their findings referred to backfills and to the absence of arching in the vertical direction behind flexible anchored bulkheads—that is, to the type illustrated by Figs. 20 and 22.

Professor Peck states that the Princeton

> "* * * measurements indicated a moderate degree of arching because the center of pressure of the curves shown in Fig. 23 is at approximately 0.38 of the distance from the dredge line to the top of the bulkhead."

The location of the resultant of lateral earth pressures might have been raised from 0.33 to 0.38 of the height not by any type of arching, but by the transfer of lateral pressures to the rigid bottom of the tank by shearing stresses along it. Furthermore, only horizontal arching could have produced the effect shown in Fig. 23 and then only provided that the anchor support had not moved. This was intentionally not the case in the Princeton tests (Figs. 15, 17, and 27).

Another statement by Professor Terzaghi[4] that should be clarified in this connection is:

> "Some of the Danish bulkheads were exposed for centuries to the pounding of waves, and for decades to heavy traffic, without the distribution of the lateral pressures becoming 'hydrostatic.'"

Originally, this statement led the writer to believe that at least some kind of pressure distribution measurements had been performed on Danish bulkheads. It is now definitely established that nothing of the kind had been done.

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[67] "Lateral Earth Pressures As a Problem of Deformation or of Rupture," by Gregory P. Tschebotarioff and Philip P. Brown, *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, Holland, June, 1948, Vol. II, pp. 81–86.

[68] "Reduction of Lateral Cohesive Soil Pressure on Quaywalls by Use of Sand Dykes," by Harris Epstein, *ibid.*, Vol. III, pp. 291–296.

[69] "Effect of Boundary Conditions on Lateral Earth Pressures," by Gregory P. Tschebotarioff and John David Welch, *ibid.*, pp. 308–313.

[70] "General Report on Section V—Earth Pressure, Stability and Displacements of Retaining Constructions," by A. E. Bretting, *ibid.*, Vol. VI, pp. 100–102.

[71] Discussion No. V610, by Gregory P. Tschebotarioff, *ibid.*, pp. 103–105.

[26] "Normer for Vandbygnings-Konstruktioner," Udgivet af Dansk Ingeniørforening, København, 1937.

[72] "Side Pressure on Retaining Walls," by Karl Terzaghi, *Engineering News-Record*, July 12, 1934, p. 55.

[4] "General Wedge Theory of Earth Pressure," by Karl Terzaghi, *Transactions*, ASCE, Vol. 106, 1941, p. 79.

A 1940 publication sponsored by the Danish Society of Engineers states[73] that the method known as the Danish rules[26] "* * * although rather complicated, is to be considered only as a rule of thumb." (The exact German word used was: "Faustregel.") The same publication[73] states that the occasion for the development of the Danish rules was "* * * provided by the light quaywall type of construction patented by the Danish firm Christiani and Nielsen." The foregoing views[70] on the matter expressed by Professor Bretting, former chief engineer of that firm, therefore take on added interest. The present chief engineer of Christiani and Nielsen, J. Brinch Hansen, published[74] in 1946 a historical review from which it follows that their first light wharves were designed "* * * by Coulomb theory but with allowable stresses of 3–4 times the stresses applying to ordinary reinforced concrete structures." This procedure is stated to have been adopted in the first place because in some old and stable timber wharves the stresses computed by the Coulomb theory were found to be from 3 to 4 times greater than usually permissible stresses.

The inescapable conclusion is that any other guess which explains the stability of structures built in accordance with the Danish rules[26] has equal validity with the assumption of vertical "arching" indicated by Fig. 22 and by the complicated formulas that support these rules. Foremost among such guesses should be the high factors of safety usually present in the structural members of a bulkhead—a point that has been discussed by Mr. Epstein in the fourth paper of the Symposium and to which Mr. Brinch Hansen has since referred in agreement.[75]

There is absolutely no contradiction between the Danish experience with full-sized structures and the Princeton experiments. After the Rotterdam conference the writer was enabled, through the courtesy of Mr. Brinch Hansen, to check the over-all design of three typical wharves and piers built by Christiani and Nielsen and found[66] that the stability was fully explained by the results of the Princeton tests.

The only previous tests of any kind from which attempts could be made to evaluate the effects of vertical arching were performed by J. P. R. N. Stroyer.[21] Messrs. Packshaw and Lake refer to these tests as "practical," presumably having in mind the simplicity of Mr. Stroyer's test setup. This simplicity was carried to such lengths that no attempt was made to measure either the distribution of lateral pressures against a 3-ft by 3-ft flexible steel plate or the distribution of its strains and deflections, and even the measurement of pressures exerted by the plate on its supports was entirely omitted. All evaluations of "arching" therefore had a purely qualitative and indirect character. They were made by comparing the forces actually applied to the overhanging ends of the plate (so as to balance the lateral pressures of the fill) to the forces theoretically necessary to balance the "Coulomb" pressures. The plate was permitted to deflect only after the entire backfill was in place. In this con-

[73] "*Zur Bemessung des doppelten Spundbauwerkes*," by I. A. Rimstad, Copenhague, 1940, p. 22.

[74] "Development of the C & N Wharf Type," by J. Brinch Hansen, *Christiani & Nielsen Bulletin No. 56*, Copenhague, 1946.

[75] *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, Holland, June, 1948, Vol. VI, pp. 107–108.

[21] "Earth-Pressure on Flexible Walls," by Jens Peter Rudolf Nielsen Stroyer, *Journal*, Inst. C. E., London, Vol. 1, 1935–1936, pp. 94–139 and 550–557.

nection Mr. Stroyer stated that his tests were intended to simulate the condition of a "sunk" wall. For that reason the writer pointed out in his paper (Part II, Section C(1)) that during the first series of the Princeton tests:

> "Arching in a vertical direction did develop after completed backfilling and an additional induced deflection between the supports. This corresponded to the small-scale tests reported by J. P. R. N. Stroyer.[21]"

Thus, Messrs. Packshaw and Lake are incorrect when they claim that the Princeton findings are at variance with the Stroyer tests. This error is probably due to the fact that the need for a differentiation between the effects of dredging in front of a "sunk" wall and the effects of backfilling behind a wall (first hinted at by Mr. Stroyer[21]) was disregarded in later publications. Thus both the Danish rules[26] and Professor Terzaghi,[22] when referring to arching in a vertical direction (Figs. 22 and 20), refer only to "backfills" and do not mention the sequence of backfilling in relation to any possible dredging—or of dredging itself, or of any other limiting factors. They most certainly do not mention relieving platforms, although Professor Bretting stated[70] at the Rotterdam Conference that the walls designed according to the Danish rules "are generally combined with a relieving platform."

Professor Terzaghi, however, must have realized the likely limiting effect of the rigidity of the anchor supports, since he included the "yield of the anchorage" in the variables to be studied by model tests of flexible bulkheads, the performance of which model tests he recommended in 1941.[4]

The sponsors of the Princeton research project were mainly interested in conditions under which any necessary dredging is done before the backfilling is started, such as are illustrated by Figs. 37 and 38 in Mr. Epstein's paper. Tests simulating such conditions were therefore given priority in the Princeton test program which, however, excluded tests with a relieving platform. A test simulating a "sunk" wall was to be conducted at the end of the program and was actually performed.

The writer devoted half a page of his paper (see Part II, Section C(1)) to emphasizing the fact that he secured diametrically opposite results concerning "arching" depending on whether the sand backfilling was done in a normal manner by dumping it through water or whether the sand was "in place at both end supports of a yielding structural unit before its final deflection." The specific statement was made with reference to the latter case that "indications were that there was arching under such conditions." Professor Peck's description of the effects of additional dredging in front of a bulkhead therefore represents an elaboration of the writer's stated point of view.

The preceding discussion has shown the flimsy experiments and theories on which conventional conceptions of arching behind bulkheads have been based. Only two specific suggestions were made in discussion. Professor Roberts suggests the "null method," which consists in varying the reaction pressure of a support in such a manner that, supposedly, it does not yield unless the support is released by a controlled amount. Very small but re-

[22] "Theoretical Soil Mechanics," by Karl Terzaghi, John Wiley & Sons, Inc., New York, N. Y., 1943, pp. 217–219.

peated reversals of the direction of support movement are unavoidable in practice, however, with alternating active and passive earth pressures being successively brought into play. Thus, an undesirable variable is introduced into the situation and justified objections to the method are offered under any circumstances. The very principle of the method precludes its application to tests of the type of the most important third series (Part III of the writer's paper), where the bulkhead was actually embedded in the ground. Professor Peck makes a similar suggestion to determine the shears at the dredge line by cutting the piling there for that purpose and by measuring the reactions necessary to hold the piling in its existing position. He admits that even in open air cuts such measurements "are not convenient." This is an understatement when applied to underwater measurements, with which discussion is now concerned. In an open cut the support needed "to hold the piling in its existing position" is provided by the opposite wall of the cut; but in the case of a full-scale anchored bulkhead it would not be possible to provide such a support without erecting an underwater obstruction to docking ships, the foundation of which obstruction, in addition, would alter the normal interplay between the active and passive earth pressures around the bulkhead. The suggested cutting of the flexible piling would also alter such interplay since divers could scarcely be expected to determine the point of contraflexure and to cut the piling there. Unless the point of contraflexure is determined, the conditions of support and of flexure of the sheet piling after the cutting would be completely different from those before the cutting. To determine the point of contraflexure under laboratory conditions by the bending-strain measuring techniques described in the paper and to add to them the cutting method proposed by Professor Peck would require the work of several highly trained men from 4 to 5 months after each test—in building and calibrating new model bulkheads and the gages mounted on them to replace the ones which had been ruined by the cutting. The duration and the cost of the testing program would then run into expenses which no sponsor could be expected to meet, especially since very little would be gained thereby.

The proposals by Professors Roberts and Peck therefore represent an effort to adapt old and inadequate techniques to a new problem, the complexity of which requires radically different and improved procedures for its practial solution. Such procedures have been provided by the new techniques of measurement and of analysis developed by the Princeton project, which have fully met the requirements of the problem at a model scale. They can also be adapted to full-scale observations.[66] The writer always was, and will be, glad to cooperate with anyone interested in such observations.

Sheet piling is designed to withstand certain maximum values of bending moment. The knowledge of the exact shape of the pressure distribution curve is therefore of practical importance to the extent that it permits the determination of anchor pulls and of bending moments. In 1948 Professor Terzaghi stated[76] that:

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[76] *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam Holland, June, 1948, Vol. VI, p. 111.

> "It is doubtful whether a definite rule regarding the pressure distribution on flexible bulkheads can be established at all which would be valid under all conditions to be encountered in practice."

Therefore the direct over-all measurement of values for design, especially of the bending moments, is of the greatest advantage since these values can be used (and in the Princeton tests actually were used[66]) for the direct comparison of test results with design values obtained from various existing theories. The corresponding measurements were performed by the writer and his staff with a precision that has thus far been unsurpassed. In no other previous test had it been possible to perform such accurate measurements by independent methods and then to check them against each other. The writer has already reported in his paper that deflections computed from the electrically measured bending strains agreed within approximately 5% with the deflections measured directly by mechanical methods. A tabulation of such comparisons, as well as an example, are given in the final report.[66] He also provided a diagram (Fig. 12) showing the reasonable agreement between the pressure curve computed from the bending-strain readings during a calibration by water pressure and the actual water pressure curve. The term "reasonable agreement" as used by the writer, of course, is intended to be relative; that is, it should be considered in comparison with the known results of other possible procedures. Direct measurements of pressures by pressure cells easily give variations up to 75% at the same spot.[77] The extrapolated over-all pressures (which are the ones that count in design) are therefore even less precise. The method of estimating the pressure distribution from the location of the resultant of two measured reactions, used prior to 1932 by Professor Terzaghi,[6] is still more inaccurate since the location of the center of pressure can have the same elevation for diametrically opposite curvatures of the pressure distribution curves with differences of from 300% to 400% of the lower values between the actual pressures in certain regions. Therefore Professor Peck's criticism that Fig. 12 shows a maximum difference of 20% between the actual and the computed (but unadjusted) values, and that near the anchor level only, is inconsistent with his use of Professor Terzaghi's outdated and infinitely less accurate technique based solely on the location of the center of pressures, to support critical arguments against the present paper.

In spite of all the data provided, it soon became evident that the Princeton techniques and findings represented too radical a departure from uncertain but time-honored procedures and concepts to be received without bias unless much more extensive documentation were supplied. Not only Professors Roberts and Peck, but also Messrs. Packshaw and Lake shook their heads about them. So did Professor Bretting[70] and Mr. Brinch Hansen[75] at the Rotterdam conference and, although in a somewhat less definite manner, so did Professor Terzaghi.[76] Professor Bretting expressed regret[70] that "no direct readings of the instruments had been given" by the writer, although this had never been done before by writers of similar papers and could not have

[77] "Earth Pressure on Tunnels," by W. S. Housel, *Transactions*, ASCE, Vol. 108, 1943, p. 1053, Fig. 46.

[6] "Record Earth-Pressure Testing Machine," by K. Terzaghi, *Engineering News-Record*, September 29, 1932, p. 369.

been done by the writer because of space limitations. Therefore the writer is greatly indebted to Messrs. R. Glossop and H. Q. Golder of London, editors of *Géotechnique*, for enabling him to announce on the spot[71] the pending publication of a paper that would do just that. This paper appeared[65] in 1948. It gives all relevant bending-strain instrument readings and subsequent lateral earth pressure computations of one typical normal backfilling test stage of test No. 54, which is one of the twenty combined active and passive earth pressure tests of the now completed third series (Part III, Section C, of the writer's paper). To do this required thirteen pages of print[65]—that is, about as much as the entire space provided for the present closure. Reference, therefore, must be made to that paper[65] and to the writer's final report[66] for a description of the numerous checks and controls developed. These checks are based on recognized laws of statics; they are much more effective and, at the same time, less crude than the measurement of reactions recommended by Professor Peck.

The writer's final report[66] gives all relevant bending-strain instrument readings and subsequent lateral earth pressure computations of two more test stages of test No. 41, the general layout of which corresponds to Fig. 70(*b*) presented subsequently. The bending-strain, shear, lateral earth pressure, and deflection curves of these two test stages are reproduced by Fig. 68. Stage V_A reflects the increased bending moments and active lateral earth pressures after the intense vibration of the backfill which was induced by repeatedly lowering on 6-in. centers a concrete spud vibrator from the backfill surface to the dredge line. Stage V_D refers to the decrease of residual passive lateral earth pressures and to the general redistribution of pressures that was induced by the subsequent intense vibration of the sand from the dredge line downward in front of the bulkhead after partial consolidation of the soft clay slope behind it (Fig. 70(*c*)).

Fig. 68 demonstrates that the procedure followed for the Princeton tests immediately and accurately reflects the changes in the condition of the soil around the bulkhead. The bending moments computed back at eight points from the adjusted pressure curves everywhere agreed closely with the bending moments as actually measured. The agreement was within 7% at all essential points. The writer's final report[66] gives sixteen additional sets of bending moment, shear, lateral pressure, and deflection curves for as many different test stages of the third series when such check backs of the computed pressure curves against the measured bending moments were performed and equally good agreement was obtained. The necessary adjustments were very slight.[65,66] This provides final and conclusive proof of the entirely satisfactory over-all accuracy of the lateral earth pressure computation procedures. Any possible refinements in the determination of actual local deviations from the computed pressure curves are completely inconsequential since they cannot produce any increase in the accuracy of bending-moment determinations which would be of any practical importance for design purposes.

Messrs. Packshaw and Lake raise a pertinent question concerning the possible effect of the grain-size distribution of the sand used on the observed breakdown of arching during backfilling operations. Only an experiment

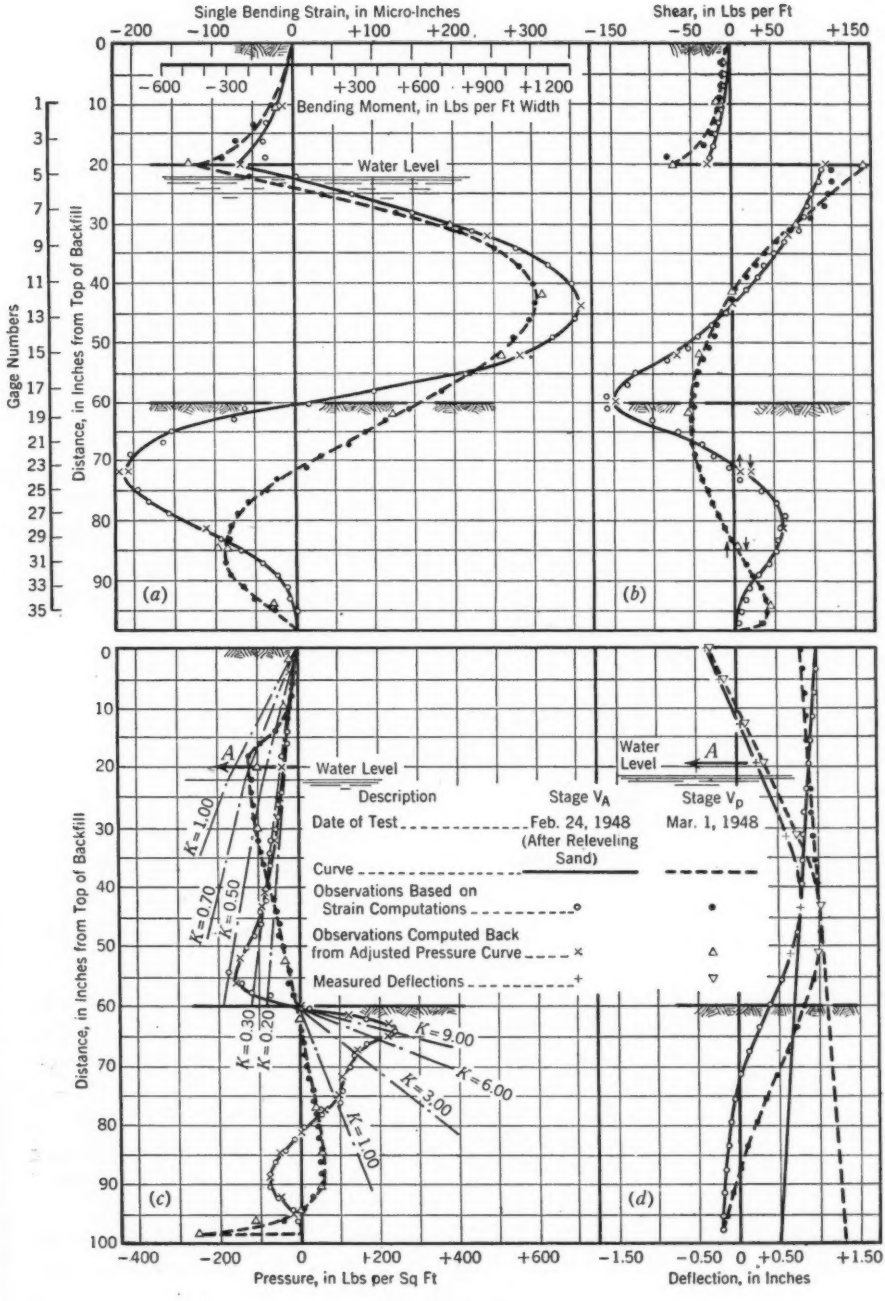

FIG. 68.—TEST 41

could conclusively settle that point; and an experiment was performed immediately. Test No. 55A was made under conditions identical to those of test No. 55, except that the usual sand was employed in the latter (which is designated as sand C) whereas a different and better graded sand D (the grading of which corresponded very closely to the grading of the sand used by Mr. Stroyer in his experiments) was employed during test No. 55A.[66] Fig. 69 shows that the over-all shape of the bending-moment and lateral earth pressure curves was essentially the same for both tests, with no evidence of arching in either case.

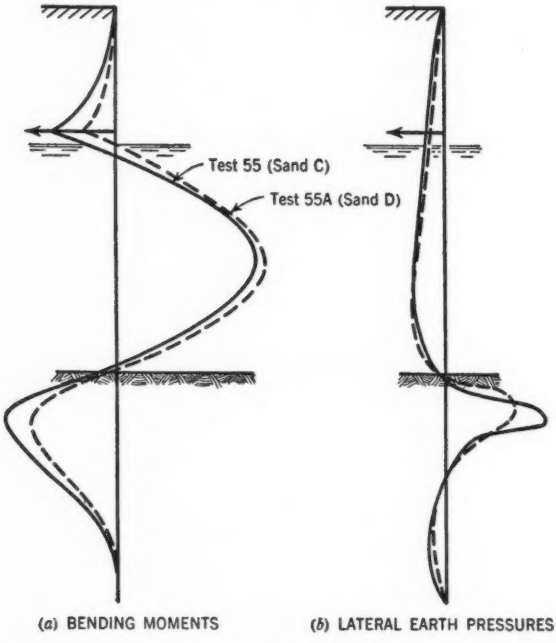

FIG. 69.—COMPARISON OF THE BENDING MOMENT AND OF THE LATERAL EARTH PRESSURE CURVES FOR STAGE IV OF TEST NO. 55 (SAND C) AND OF TEST NO. 55A (SAND D)

The model similarity factors discussed by Professor Krynine had been fully taken into account during the tests and their subsequent evaluation. The distances between dredge and anchor levels, as well as other dimensions, were changed in different tests until complete model similarity of deflections was obtained.[65,66] The effect of the **Z**-shape of actual sheet piles, as compared to the rectangular shape of the model bulkhead plates (which were $\frac{1}{4}$ in. thick during the third series), was taken into consideration by a corresponding decrease of bending stresses on the model.

Professor Krynine advances the suggestion that the degree of yielding in the wall should be expressed in absolute values "rather than in fractions of the span of the wall as usually done." This idea may be properly questioned since shearing stresses depend on the magnitude of angular deformations and not on

linear displacements. The writer knows of no published data that would corroborate Professor Krynine's suggestion, and no observations have been made in the Princeton laboratory that would support it. On the other hand, Mr. De Beer's suggestion that "* * * even sands have some interlocking cohesion c (generally about 0.2 ton per sq m)" has the opposite implication. This view is also contradicted by the results of numerous lateral sand pressure tests at different model scales. Subsequent personal correspondence with Mr. De Beer revealed the fact that his statement was based on indirect reasoning and mainly on the results of triaxial cell tests when a straight line drawn tangent to the Mohr circles did not pass through zero. The writer attributes this observation to the action of the rubber envelope which encloses the soil sample in such tests.

The finding that lateral earth pressures decrease during consolidation of fluid clay backfills, with no outward bulkhead movement being necessary to achieve this reduction, is considered by Professor Peck as "quite normal." Other discussers take a similar attitude. The fact remains, however, that this phenomenon had not previously been so described in modern soil mechanics literature and that the similar, although only qualitative, results obtained by R. O. Comer[58] in 1923 have been generally overlooked. Mr. Feld should be commended for drawing attention to them now.

Messrs. Packshaw and Lake still believe that a movement of more than 1.5% of the height of the wall might reduce the "at rest" pressures of clay backfills to their "active" pressures in accordance with Professor Terzaghi's purely theoretical application of the conventional stress-strain concepts to that problem.[11,12] Professor Peck takes a similar position when he states that after a consolidated equilibrium condition has been reached: "* * * further outward deformations would be necessary to establish a state of failure in any region of the backfill, * * *." The writer, on the other hand, considers on the basis of his tests that the very terms "at rest," or "neutral," or "consolidated equilibrium" imply a limit state of incipient failure where plastic cohesive backfills or remolded natural clays are concerned. This point of view is supported by Mr. De Beer's reference to the results of numerous cell tests performed in Holland and in Belgium. The Department of Public Works of Amsterdam, Holland, indirectly provides further support[78] of the writer's independent findings on this matter[79] by stating: "According to Keverling Buisman[80] however the neutral pressure in cohesive soils approaches the active pressure." The need to recognize the limitations of conventional stress-strain concepts as applied to cohesive soils is therefore definitely indicated.[71]

Mr. De Beer points out an inconsistency in part of Fig. 19. This was caused by a "slip of the pen" which the writer regrets. When extrapolating

[58] *Transactions*, ASCE, Vol. LXXXVI, 1923, p. 1555.

[11] "A Fundamental Fallacy in Earth Pressure Computations," by Karl Terzaghi, *Journal*, Boston Soc. of Civ. Engrs., April, 1936, p. 87.

[12] "Theoretical Soil Mechanics," by Karl Terzaghi, John Wiley & Sons, Inc., New York, N. Y., 1943, p. 96.

[78] "Earth Pressure on Horizontal Circular Conduits" (Dept. of Public Works, Amsterdam), *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, Holland, June, 1948, Vol. III, pp. 317–320.

[79] *Ibid.*, Vol. VI, pp. 108–111.

[80] "Grondmechanica," by Keverling Buisman, 2e Druk, Delft, 1943.

the curves below the last measured values at the "K_B"-level, their curvatures should be reversed. This correction, however, only serves to emphasize further the great restraining effects of the underlying rigid boundary.

The writer indicated the likelihood of that effect in Fig. 23 by drawing a hypothetical active earth pressure curve of lesser intensity which he suspected might develop behind bulkheads actually embedded in the ground. Mr. Feld believes this should have been omitted. The writer, however, believes that it is desirable to indicate the hypothesis on which further research in progress is based, especially if this warns of possible limitations of previous findings and if its hypothetical nature is clearly indicated—as it was, both in words in the text and by a question mark on the drawing.

The tests of the third series were undertaken not because there were any deficiencies of the previous installation or techniques as such, but because it became evident that an over-all solution of the problem could be obtained only

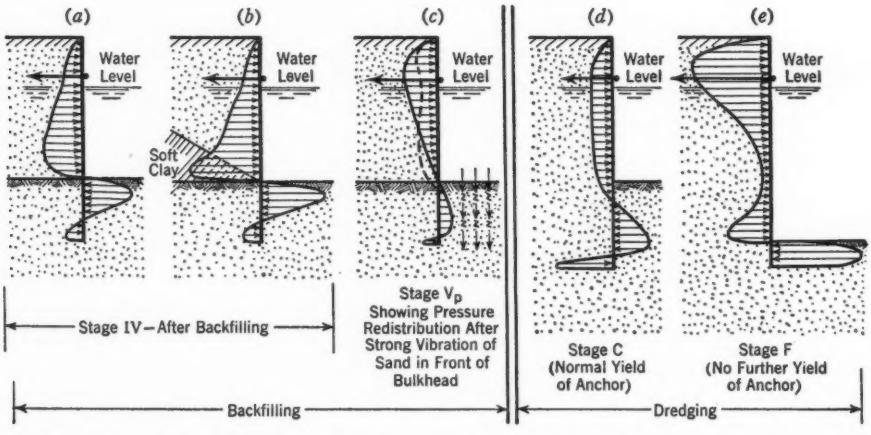

FIG. 70.—FIVE MAIN LATERAL EARTH PRESSURE DISTRIBUTION TYPES DETERMINED DURING THE PRINCETON TESTS WITH MODEL FLEXIBLE BULKHEADS

by a new approach which had never been possible before in any detail—that is, by the simultaneous study of active and passive earth pressures and of their interplay. The results confirmed and extended the findings of the first series as well as the suspected effect that had been indicated by a question mark in Fig. 23.

Fig. 70 outlines in sketch form some of the findings of the twenty combined active and passive earth pressure tests of the third series performed in 1948, a few of the main features of which can be summarized as follows:

(1) Vertical arching, because of the flatness of the "arches," is the least stable of the three possible main types. It is similar to horizontal arching in that it cannot noticeably develop behind an anchored bulkhead when backfilling is done (Figs. 70(*a*) and 70(*b*)) because no soil is present during most of the backfilling to provide an abutment for the arch at and above the anchor level, and because the elastic elongation of the anchor and the yield of its supports

progressively increase as backfilling goes on. The yield induces the breakdown of any arches.

(2) Definite evidence of vertical arching was obtained at the Princeton laboratory only during the stages of a test that simulated dredging in front of a "sunk" wall, the anchor support of which did not yield any further (Fig. 70(*e*)). Of all the theoretical pressure distribution curves so far advanced which assume arching, only the one proposed by J. Ohde[73,81] closely corresponded to the distribution determined at Princeton under the foregoing conditions. This distribution appears to be caused by a simultaneous action of passive soil resistance above anchor level and of arching both in the horizontal and in the vertical directions. The distribution suggested by the Danish rules (Fig. 22) and by Professor Terzaghi (Fig. 20) does not appear possible under any conceivable conditions.

(3) An absence of lateral displacement at the anchor level is conceivable on actual structures only when the sheet piling is connected to massive relieving platforms. Since the presence of numerous piles under a relieving platform has an unknown but definitely not negligible effect on the redistribution of earth pressures against the sheet piling, it is still an open question whether arching can occur under field conditions even in the case of a "sunk" wall.

(4) The residual passive pressures of sands below the dredge line in front of the bulkhead reached values which in some cases were from three to four times greater than the maximum values that are theoretically possible if the effect of wall friction is neglected. Effective restraint below dredge level was recorded even when the entire embedded length of the bulkhead had moved outward from its original position.

(5) The point of contraflexure of bulkheads embedded in sand coincided (after normal backfilling) very closely with the dredge line. Of all existing bulkhead design methods H. Blum's "equivalent beam" version[82] of the fixed earth support method[22] gave the best agreement with the observations made for backfilling conditions.

(6) Severe vibrations and other causes frequently induced an appreciable downward displacement of the point of contraflexure. However, no correspondingly large increase of bending moments was observed because a considerable simultaneous additional transfer of lateral pressures to the soil beneath the dredge line appeared to take place as a result of the bulkhead deformations which lowered the point of contraflexure. The transfer increased with the flexibility of the bulkhead, on which its displacement at dredge line level directly depended.

In view of these findings, it does not appear advisable to attempt, when making new designs, the actual determination of all possible variations of pressure distribution. Instead the writer proposed,[83] for all designs involving clean sands, a simplified version of Mr. Blum's "equivalent beam" method[82]

[81] "Zur Theorie des Erddruckes unter besonderer Berueksichtigung der Erddruckverteilung," by J. Ohde, *Die Bautechnik*, Berlin, Nos. 10/11, 13, 19, 25, 37, 42, 53, and 54, 1938.

[82] "Einspannungsverhaeltnisse bei Bohlwerken," by H. Blum, Ernst und Sohn, Berlin, 1931.

[83] "Design of Flexible Anchored Sheet Pile Bulkheads," by Gregory P. Tschebotarioff, *Proceedings*, 17th International Navigation Cong., Lisbon, Portugal, 1949.

under full utilization of maximum possible wall friction both for active and passive pressures. Empirical coefficients are suggested to take into account possible variations in the maximum bending moments and in the anchor pulls when uncertainties in the support conditions are known. The proposed design procedure corresponds closely[66] to the bending-moment values directly measured during the third series of tests at Princeton, which are approximately equal to one half of the theoretical bending moments obtained by the free earth support method[22] for the same depth of embedment and which are some 25% higher than the values obtained by the Danish rules.[26,73] Therefore, although the pressure distribution determined at Princeton for normal backfilling is exactly the reverse of the one suggested by the Danish rules, the writer pays tribute to the authors of these rules as good guessers concerning the permissible limit reductions in conventional design values, even though the actual factors of safety appear to be smaller than indicated by these rules.

In addition to the bulkhead design procedure for use with clean sands. the writer's final report[66] (*a*) suggests a design procedure for use when clays are involved; (*b*) brings out a number of secondary but nevertheless essential observations; (*c*) provides extensive documentation of observations made; (*d*) records not yet clarified points; and (*e*) makes specific recommendations for the techniques of full-scale control observations. It is suggested that control measurements be made on all new bulkheads.

In closing, the writer wishes to express once more his sincere appreciation to the United States Department of the Navy for the sponsorship of the extensive test program described and to the officers and engineers of the Bureau of Yards and Docks as well as to the staff members of Princeton University who have helped him bring it to a successful completion.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

FLEXURAL CONSTANTS OF HAUNCHED BEAMS BY AREA COMPUTATION

Discussion

BY WILLIAM A. CONWELL

WILLIAM A. CONWELL,[20] M. ASCE.—A comparison of this paper with a current paper entitled "Beam Deflections by Second and Third Moments" by Hsu Shih-Chang[21] leads to the conclusion that, although the papers treat different subjects, their underlying philosophy is the same. In each instance there is a preference for what might be termed the mechanical method of determining the moment of a load or an area—that is, multiplying the load or the area by its distance from the point about which moments are being taken. In view of the prevalence of the mathematical viewpoint of considering moments as the integral of shears and, in most instances, the convenience of so handling the problem, the use of fundamentals only by these authors is most interesting. Perhaps in his closing discussion, Professor Tsai would like to comment on this point. It would be of interest to know whether this approach represents a return to fundamentals by engineers in China or whether they never left them.

Even though novel methods, such as the method suggested by the author may not immediately replace others now in vogue, it is important that they receive consideration and be published. Only by charting these new paths is it possible to stimulate thought, strike a sympathetic chord in the minds of others thinking of the subject, and perhaps, lead the way in the development of new and powerful methods.

The writer believes that the author, unduly and modestly, limited the scope of this paper in his choice of title. The implications and the content of the text carry far beyond the field of haunched beams. Likewise, the reasons Professor Tsai gives for advancing his theses are stated only in terms of its advantages over existing procedures and do not touch the tremendous imaginative appeal inherent in these new thoughts.

NOTE.—This paper by Fang-Yin Tsai was published in September, 1948, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1948, by J. Edmund Fitzgerald, and Harris Solman; February, 1949, by Robert V. Hauer; and March, 1949, by George Epps.

[20] Gen Engr., Structural Eng. and Design Dept., Duquesne Light Co., Pittsburgh, Pa.

[21] "Beam Deflections by Second and Third Moments," by Hsu Shih-Chang, *Transactions*, ASCE, Vol, 113, 1948, p. 445.

In his review of existing methods in the "Introduction," the author should mention a possible extension to the trapezoidal method which almost completely overcomes his objection to it. By making the basic assumption used in the development of Simpson's rule (which the author recommends in his method), it is possible to obtain corrections to the trapezoidal diagrams which bring them very close to the actual diagrams. This assumption (that the curve is a second degree parabola between any three consecutive points) yields a substitute diagram of trapezoids and corrective parabolic segments. Since such a substitute would closely follow the actual curve and since the properties of the geometric figures involved may be quickly determined, it would appear that a much higher degree of accuracy than the author implied can be obtained with little effort.

In his "Introduction," the author mentions the "many comprehensive tables and charts" of flexural constants which are available for common types of beams. It is now generally accepted that advances in knowledge in this field will be marked by a gradual accumulation of data in chart and tabular form until common types are completely covered. The "Handbook of Frame Constants, Beam Factors, and Moment Coefficients for Members of Variable Section,"[22] prepared by the Portland Cement Association, with its mass of data for straight and parabolic haunches of varying lengths, symmetrically and unsymmetrically placed would seem to be close to the ultimate authority in this regard. Although the author seems to think of his work as being applicable to the more uncommon type of beam, it is apparent that it could be valuable in the extension of tables and charts as more types of beams become sufficiently common to warrant the expression of beam characteristics in this form.

The very fact that certain types of beams may be designated as uncommon leads to the conclusion that they occur so infrequently that the development of of special methods for their analysis would scarcely seem justified. Should some of the uncommon types be of sufficient importance, of course, tabular data should be developed for them.

Although the author stresses the application of this method to unusual types, he chooses for his numerical examples, types which are covered by tables. He quotes results from the Russell tables[13] which check the computations by momental areas. It would be of value to know if the data from those tables were read directly or if interpolation was required. The writer had to use interpolation to check the results with the Handbook. A comparison of the author's coefficients with those obtained by interpolation from the Handbook are shown in Table 3 for Examples I and II. This set of coefficients shows that, in general, linear interpolation of Table 3 yields results within the accuracy normally required for engineering design. The maximum error occurred in Example II where a 5.3% variation in the coefficient for $M_{fo(1)}$ was noted. When parabolic interpolation was employed, the Handbook coefficient of $M_{fo(1)} = 0.1835$ in Table 3(*b*) became 0.1930; and the error, 0.4%. The para-

[22] "Handbook of Frame Constants, Beam Factors, and Moment Coefficients for Members of Variable Section," Portland Cement Assn., Chicago, Ill., 1947.

[13] "Analysis of Continuous Frames," by E. B. Russell, San Francisco, Calif., 1934 (lithographed), pp. 24–29.

bolic interpolation was based on values for the three points nearest the point for which a value is desired. A similar improvement in accuracy can be obtained at any time when there is doubt as to the efficacy of straight-line or trapezoidal methods.

TABLE 3.—COMPARISON OF COEFFICIENTS AS COMPUTED IN THE TSAI METHOD AND AS GIVEN IN THE "HANDBOOK OF FRAME CONSTANTS, BEAM FACTORS, AND MOMENT COEFFICIENTS FOR MEMBERS OF VARIABLE SECTION"[a]

(*a*) EXAMPLE I						(*b*) EXAMPLE II		
Item	Tsai	Handbook	Item	Tsai	Handbook	Item	Tsai	Handbook
C_{on}	0.404	0.405	$M_{fn(2)}$	0.0647	0.0652	C	0.651	0.646
C_{no}	0.834	0.832	$M_{fo(2)}$	0.1251	0.1246	S'	1.96	1.96
S'_o	2.44	2.44	$M_{fn(5)}$	0.0820	0.0858	$M_{fn(1)}$	0.0746	0.0754
S'_n	1.18	1.18	$M_{fo(5)}$	0.1088	0.1078	$M_{fo(1)}$	0.1938	0.1835
$M_{fn(1)}$	0.1243	0.1203	$M_{fn(6)}$	0.0472	0.0478	$M_{f(2)}$	0.0987	0.0991
$M_{fo(1)}$	0.1343	0.1309	$M_{fo(6)}$	0.1416	0.1447			

[a] "Handbook of Frame Constants, Beam Factors, and Moment Coefficients for Members of Variable Section," Portland Cement Assn., Chicago, Ill., 1947.

As long as engineers analyze structures, there will probably be debate as to the types of functions that should be taken as flexural constants. The author makes a strong case for the use of end angle changes on the basis that they assist in visualizing the action of the structure. There is no questioning the desirability of visualizing structural action. Hardy Cross, Hon. M. ASCE (who, because of his work in moment distribution, may be said to be a proponent of the use of end moments for flexural constants), took cognizance of the importance of such visualization when he wrote:[23]

> "The ability of a designer of continuous structures is measured chiefly by his ability to visualize the deformation of the structure under load. If he cannot form a rough picture of these deformations when he begins the analysis he will probably analyze the structure in some very awkward and difficult way; if he cannot picture these deformations after he has made the analysis, he doesn't know what he is talking about."

This statement implies that a knowledge of moments should enable a designer to visualize the deformations of the structure and such is the case. Since moments are required for design, they must always be computed and there is thus a good argument for making use of them as flexural constants and later determining such actual deflections as may be required. To the writer and many others this procedure seems more logical than first determining deformations that may or may not be required and then finding the moments that will, of course, be required for design.

[23] "Continuous Frames of Reinforced Concrete," by Hardy Cross and N. D. Morgan, John Wiley & Sons, Inc., New York, N. Y., 1932.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

SEA LEVEL PLAN FOR PANAMA CANAL

TO PROVIDE MAXIMUM SAFETY AND UNLIMITED CAPACITY

Discussion

BY JOHN G. CLAYBOURN

JOHN G. CLAYBOURN,[20] M. ASCE.—Since the presentation of this paper at the Annual Meeting of the Society in New York, N. Y., on January 16, 1946, a

> "* * * comprehensive review and study, with approximate estimates of cost, of the means of increasing the capacity and security of the Panama Canal to meet the future needs of interoceanic commerce and national defense * * *"

has been concluded in accordance with the provisions stipulated under Public Law No. 280.

These investigations and studies were conducted under the immediate direction of Colonel Stratton, supervising engineer of the Panama Canal and under the over-all supervision and review of a Board of Consulting Engineers—all appointed by the Governor of The Panama Canal (J. C. Mehaffey) especially for the purpose. Only one member of this group had ever been associated with the canal organization previously—namely, General Kramer, who was supervising engineer of the Panama Canal when the Third Locks construction was terminated in May, 1942, and a lock type advocate at that time.

The studies made by this group were the most comprehensive ever made of the Panama Canal and these data, together with the conclusions of the Board of Consultants (which were unanimous), were reviewed, approved by the Governor, and included in his report to the President of the United States, Harry S. Truman, through the Secretary of Defense, James V. Forrestal, in October, 1947.

The unanimous conclusions reached in brief were:

NOTE.—This paper by J. G. Claybourn was published in February, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1948, by Fred Lavis and Raphael G. Kazmann; and June, 1948, by Hans Kramer, and James H. Stratton.

[20] Gamboa, Canal Zone; formerly Supt., Dredging Div., The Panama Canal, Gamboa, Canal Zone.

(*a*) The Canal as it exists today or any lock type canal constructed to have maximum security feasible cannot be considered as meeting the needs of national defense;

(*b*) Only a sea level canal will satisfy conditions prescribed under Public Law No. 280; and

(*c*) Such a canal can be built most economically at the site of the present waterway.

A thorough review of the Governor's report will furnish unbiased, unequivocal, and incontestable answers to previous controversial issues. These conclusions are based on facts and on calculations that may be reviewed and checked; and then are backed by a wealth of experience and have been confirmed by model tests in all essential particulars.

Since the United States first became interested in the Panama Canal, the value of that project as an instrument of defense has always been held of primary importance, transcending commercial requirements or necessities, by the United States Government. The Canal was built primarily as an instrument of defense; it still is the greatest single instrument of defense and it must remain as such, as long as sea power and water transportation are vital to national safety.

The concept of a two-ocean, three-ocean, four-ocean, or five-ocean navy, each with its auxiliary supply, hospital, transport, and other ships to support and supply these several units independently and successfully, is mere twaddle even to the layman, who witnessed the shuttling of war vessels, and their supporting supply ships, through the Canal, to the side most needed, throughout World War II. The axiom of Gen. Nathan B. Forrest—"The side wins that gets thar fustest with the mostest"—was knowingly or unconsciously followed at all times. This was possible because, by good fortune, the Canal was not attacked and was available. Had the Canal suffered an attack, such as at Pearl Harbor (Hawaii), results may well have been more disastrous than the loss of the fleet and, with proper timing, both could have been easily destroyed. By virtue of the Panama Canal, no one but the "Top Brass" eventually knew where or how many fleets the United States had since the number and the place were changed overnight.

It will always be necessary to have a canal available to transfer warships and supply ships from one side of the United States to the other, in the shortest possible time, during any war in which the nation may be engaged.

It is apparent that, if the item of defense is omitted from the requirements of Public Law No. 280, the problem would be simple because, as Colonel Stratton has stated:[19]

> "The present lock canal could be improved at a cost of $129,983,000 to meet the needs of commerce for the remainder of the twentieth century. A lock canal designed to meet the future needs of commerce and constructed to have the maximum security feasible in this type of canal would require new locks and strengthened summit lake impounding dams. It would cost $2,307,686,000 and would still be deficient in resistance to modern weapons."

[19] "The Future and the Panama Canal," by James H. Stratton, in "Panama Canal—The Sea-Level Project: A Symposium," *Proceedings*, ASCE, April, 1948, p. 444.

A sea level canal—an enduring and indestructible item of defense—as estimated in the Governor's report, would cost $2,483,000,000.[19] This cost is but $185,314,000 more than that of the lock type canal "constructed to have the maximum security feasible" which is still vulnerable to modern weapons. Besides, the vital works of a lock type canal—dams, spillways, locks, and their necessary operating machinery—are most vulnerable and easy to sabotage. Conversion will require at least ten years and probably more. The project will never be ready if a start is not made.

The foregoing discussion will answer Mr. Lavis' query as to the "* * * valid reason for considering a so-called sea level canal at Panama at this time."

As to economic justification: From a purely commercial angle the Panama Canal has not yet justified expenditures for its construction except for the boom years preceding the crash of 1929 and for one or two years since, when full interest on the investment and some so-called amortization, apart from the actual cost, was paid. Some intangible benefits would be difficult to evaluate in dollars. There are also good prospects that commercial activities will become profitable in the future, should world conditions stabilize, thus permitting South American trade development. However, in wartime the cost of the Canal has "paid off."

When the United States declared war on December 8, 1941, commercial traffic, as such, stopped or was curtailed in response to the necessities of war. It is significant that the number of ships and the gross tonnage that passed through the Canal during World War II are very comparable to those during the greatest peacetime commercial traffic years. The relative revenues may be computed and would be a persuasive argument, but the actual monetary value of transferring vessels of war, troopships, Army and Navy supply ships, and other vessels to supply materials necessary to the war effort of the United States and its allies is inconceivable and incalculable.

At the rate per day that World War II was costing the United States, the de luxe sea level canal proposed by the Governor in his report of October, 1947, would require the approximate equivalent of ten days of such expenditure. The cost of future wars will probably be greater. It is inconceivable to anyone familiar with the tremendous transportation facilities furnished the war effort by the Panama Canal, that this contribution did not shorten both World Wars I and II materially; and, obviously, it also saved the lives of thousands.

The war value of the Panama Canal is continuous. However, only by conversion to sea level, can the Panama Canal—this key to national and hemispheric defense—be made enduring and indestructible and, in the words of the Hon. Joseph J. Mansfield, "It is one defense item that cannot become obsolete and will be a lasting benefit in war or peace." If the Canal serves its purpose of defense on one critical occasion, its cost will have been justified.

Mr. Lavis states that:

> "The writer has seen the old channel of the Chagres River in Panama. He will readily acknowledge the ability of engineers, contractors, and modern excavating machinery, but he has a very high regard for the difficulties of the Culebra-Gaillard cut—more so, it would appear, than has the author of this paper."

The writer's "regard for the difficulties of the Culebra-Gaillard cut" is based on nearly thirty-eight years of experience on Panama Canal excavation, during and since construction, twenty-six years of which were as head of the Dredging Division. This service involved immediate supervision as well as general supervision of excavation over every section of the Canal from one end to the other; and one of his duties was the removal of slides and their prevention. Under the writer's supervision the Dredging Division accomplished both tasks. There have been no serious slides on the Canal since 1932, even at Culebra, the the area most susceptible to slides. Potential slide areas still exist and slides may still occur, because remedial treatment advocated and used by the writer, has not been applied in many sections, awaiting the outcome of Congressional action on future canal development.

Charges up to 15 tons of 60% sodium nitrate dynamite have been exploded at depths of 75 ft in areas most susceptible to slides without starting, or precipitating, a slide and on slopes that had not attained the calculated angle of repose. It was noted from pictures taken at the focal points of the atom bomb explosion at Hiroshima, Japan, which exploded above ground, that, although all structures in the vicinity were disintegrated, the conditions and outlines of the river channels through the city remained unchanged. It is logical to assume that even an atom bomb, exploding on the surface of an ample predetermined slope (a thirty-year slope shaped in nature's laboratory—the banks themselves), would not cause an appreciable slide; nor would an atom penetrating bomb be sufficient to close a channel of 500 ft or more. The soils mechanics and atom bomb experts apparently are of the same opinion.

Mr. Kazmann comments on the writer's citation of a hypothetical statement by Mr. Abbott[4] as of December, 1944, "* * * six months before the first atomic bomb was detonated in the New Mexico desert." That hypothetical statement is now considered a statement of fact by the Board of Consultants in its study under Public Law No. 280. It has also received the approbation of the Governor in his report to the President as well as that of at least two former commanders-in-chief of the Caribbean Command (Generals George H. Brett and Willis D. Crittenberger) and two former governors (Generals Julian L. Schley and Glen E. Edgerton, M. ASCE) of the Canal Zone who are fully acquainted with conditions on the Canal.

From the foregoing discussion it should be gathered that the Panama Canal is a worthwhile target. It is admitted that if, the United States were destroyed, there would be no use for any canal, lock or sea level. However, present conditions should not be permitted to continue merely on the premise advanced that the "proposed engineering improvement" might be "nullified" by some probable future "fundamental technologic change."

It is the opinion of the Board of Consultants that the sea level canal, proposed by the Governor, can operate with very little danger of closure, except for short periods, under the worst conditions of war now known and will be tremendously more safe from destruction from any future and unknown perils that may arise, than the present lock canal.

[4] "The Hypothetical Sea-Level Project," by E. E. Abbott, memorandum to the Engr. of Maintenance, The Panama Canal, Records Bureau, Balboa Heights, December 2, 1944.

At Bikini, radioactive vessels that were not sunk in the lagoon were towed to sea and destroyed. The same procedure would be followed at the Panama Canal. Contaminated water would be flushed out by tide wash; a sunken hulk could be dug around or blown up and removed by derricks.

Locks, dams, and spillways would be the targets of any bombing attack on the lock canal at Panama, conventional or atomic. A hit or a near miss with the atomic bomb would demolish or breach these structures; the lake would be emptied and there would be no canal. Should the vital canal structures remain intact and flushing operations be undertaken from Gatun Lake to clear the summit level from radioactive waters, the lake would probably be drained in the process and there would be no canal, because very little current would be produced, with the sluice gates of the spillway and lock culvert valves wide open, at a point far enough away from an atomic bomb explosion to escape damage to the vital lock canal structures.

With the sea level canal, radioactive waters would be dissipated by tidal flow four times in 24 hours, if the tidal gates were destroyed, and by the same method, but more scientifically, if they were not destroyed.

When fogs occur in the cut section of the Panama Canal, they develop from about 11:00 p.m. to 11:30 p.m., and are usually completely dissipated from 7:00 a.m. to 7:30 a.m. If there is an appreciable wind, fogs will not form. If rain occurs after the fog has formed, the fog is cleared or precipitated by the rain. If the sun hits the fog before the wind currents begin in the morning, the fog is precipitated. If wind currents form early (which is often the case) and from the north, the fog is blown out of the cut. All fog particles passing the divide, between Gold and Contractor's hills are dissipated within approximately 2,500 ft south of that divide. Any ill effects from fog contamination would be about the same for a lock canal or a sea level canal, but would diminish in direct proportion to the lessening of ill effects of the contaminated water and would be in favor of the sea level canal.

The writer was gratified that the plan finally approved by the Governor in October, 1947, and arrived at after intensive study by the supervising engineer and the Board of Consulting Engineers, was basically the same as that covered by the paper.

The same flood control scheme, which operates independently of canal channels, was adopted with minor changes—that is, Madden Dam was retained in operation as an additional flood control item; a tunnel was provided from the Gatun Reservoir through the high ridge to the East Diversion cut around the Gamboa Hills, and a baffle system was introduced in the East Diversion.

It is suggested that a review be made of the writer's suggestion to utilize the present Gamboa Reach for this section of the East Diversion, now that this part of the present Canal would be abandoned, thus saving considerable excavation and cost.

Instead of following the route of the lower lying Chagres River Valley and that of the tributary of the Obispo through the cut section, as in the writer's plan, a new alinement of the Canal from immediately north of Pedro Miguel, almost direct to Gatun was adopted. Reaches, each nearly 5 miles in length,

a ruling width of 600 ft, and turns at intersections not exceeding 26° are provided. Wetted surfaces are planned with ample slopes giving usable maneuvering room for the full width of channel for vessels and insuring safety to transiting ships.

Costs submitted for the Governor's plan substantiate the data published by the writer when evaluated on yardage involved. The items of difference are:

Description	Governor's plan	Claybourn plan
Width of channel, in feet	600	500
Depth, in feet (Panama level datum)	−60	−50
Curves, in degrees (maximum)	26	....
Cutoffs	Complete realinement	....

In the writer's plan the total curvature (not stated) would depend on model tests. The plan for cutoffs was to follow the present route except for a cutoff south of Mamai Curve and Gamboa Reach, another cutoff at Bohio, and one from Peña Blanca Reach to the site of the new Gatun Lock.

To summarize:

(*a*) The sea level canal proposed by the Governor in his report of October, 1948, fulfils, in all details, the requirements of Public Law No. 280 and only such a canal would be secure against a determined and resourceful enemy attack with modern weapons.

(*b*) The Canal as it exists today cannot be considered to meet the future needs of commerce or national defense.

(*c*) A sea level canal can be built most economically at the site of the present waterway.

(*d*) The proposed sea level canal does not depend on an artificial lake for its water supply, which, according to General Edgerton, offers the "greatest danger of prolonged closure of the present canal to enemy action," and could still operate even if the tidal locks were destroyed.

(*e*) The sea level canal will provide for all future commercial needs.

(*f*) With tide gates open on the 600-ft canal, the maximum current that will be experienced is 4.5 knots, and ships navigate the 500-ft Cape Cod Canal, in Massachusetts safely with currents as rapid as 6 knots.

(*g*) The invulnerability of a sea level canal has been demonstrated at Suez (Egypt). Its serviceability has been proved by the fact that such a canal passes the largest ships efficiently, at low cost, with practically no hazard and through channels only 270 ft wide, less than half the width recommended at Panama. The Suez Canal was blocked by sunken ships during World War II, but only for days, not for months or years.

(*h*) An average of 2.50 hours of transit time would be saved by the sea level canal as compared to the best lock type that could be built.

(*i*) The sea level canal will provide the United States with an enduring and indestructible item of defense.

(*j*) A canal must always be available to transfer ships of war, supply ships, transports, etc., from one side of the United States to the other in any war in

which the nation may be engaged, and only a sea level canal will satisfy these requirements.

(*k*) The sea level canal is entirely feasible and practicable.

(*l*) The only advantages the lock canal had over the sea level type, at the time of construction, were lower first cost and earlier completion date. It served its purpose, however, and justified its construction in World War I. The canal is now obsolete and must be converted to sea level to continue to serve the purpose for which it was constructed.

(*m*) The lock canal is not only vulnerable to air attack, but the operating machinery on which it depends is easily sabotaged.

(*n*) The cost of a sea level canal is but a fraction of what the nation will spend on items of defense. The canal is one item that will not become obsolete, and it will furnish unlimited capacity and maximum safety in peace or war for ships of the United States.

(*o*) It is far better and more economical to build one invulnerable sea level canal than to build another lock canal elsewhere. In the latter case, neither one nor the other could be defended.

(*p*) The war value of the Panama Canal is continuous but can be realized only if converted to sea level.

(*q*) A curtailment of any future war by ten days, by virtue of having a dependable canal, is justification for the conversion to sea level. The United States will have secured unlimited capacity for peace and for all time, in any event, which would not be achieved by the optimum lock type canal.

(*r*) It is the opinion of all high military officials that the Canal is a vital part of the nation's defenses. It will take at least ten years to make the conversion. The sooner the work is begun, the sooner it will be finished.

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DISCUSSIONS

PANAMA CANAL—THE SEA-LEVEL PROJECT
A SYMPOSIUM

Discussion

BY JAMES H. STRATTON; RALPH P. JOHNSON AND SYDNEY O. STEINBORN; F. S. BROWN; J. S. MEYERS AND E. A. SCHULTZ; C. A. LEE AND C. E. BOWERS; J. E. REEVES AND E. H. BOURQUARD; WILSON V. BINGER AND THOMAS F. THOMPSON; A. CASAGRANDE AND W. L. SHANNON; AND J. J. ROSE, F. L. DYE, W. B. WATSON, AND L. T. CROOK

JAMES H. STRATTON,[137] M. ASCE.—In this closure to the Symposium, the writer wishes first to acknowledge on behalf of all the authors their gratitude for the generous attention given by the discussers to their efforts to compress within reasonable bounds a description of the extensive investigations and studies undertaken by a fairly large engineering organization over a period of nearly 2 years. Several discussers have evidently misunderstood the main purpose of the Symposium, which was to present to the Society for discussion and criticism the engineering features of the recommended plan. In consonance with this purpose only enough information was included in the Symposium regarding the reasons for adopting the plan to serve as a background for the presentation; and a complete discussion of those reasons, even if it were appropriate in an engineering paper, would have been impossible because of security requirements. The selection of material for presentation in the Symposium papers necessarily involved omissions which, no matter how discriminating, could not fail to create the impression that there were corresponding omissions or oversights in the studies. Unfortunately, in correcting this impression where it is in error, space limitations do not permit more than a passing

NOTE.—This Symposium was published in April, 1948, *Proceedings*. Discussion on this Symposium has appeared in *Proceedings*, as follows: June, 1948, by Hans Kramer, and Philip G. Nichols; September, 1948, by George B. Pillsbury, Kenneth S. M. Davidson, Joel D. Justin, W. H. McAlpine, W. E. R. Covell, William Herbert Hobbs, Hibbert Hill, Kenneth C. Reynolds, Gregory P. Tschebotarioff, Charles W. Dohn, and Donald F. Horton; October, 1948 by E. Montford Fucik, Charles M. Romanowitz, and Raphael G. Kazmann; November, 1948, by George B. Massey, William Allan, and Boris A. Bakhmeteff; December, 1948, by Robert C. Sheldon, F. W. Edwards, and H. R. Cedergren; January, 1949, by Harry O. Cole, Ole P. Erickson, Clarence S. Jarvis, Anson Marston, and Ralph Z. Kirkpatrick; February, 1949, by Ernest Shankland, and John S. McNown; and March, 1949, by H. H. Little, and A. Casagrande.

[137] Col., Corps of Engrs., U. S. Army, Div. Engr., New England Div., Boston, Mass.; formerly Supervising Engr., Special Eng. Div., The Panama Canal, Diablo Heights, Canal Zone.

reference to this fact. For those who are interested, a complete copy of the Governor's (J. C. Mehaffey's) report on The Panama Canal has been placed in the Engineering Societies Library.[137a] The record of these discussions should serve as an invaluable guide in the reevaluation of certain phases of the investigations during the design and the construction of the project when authorized by Congress.

In most instances where discussers have challenged the conclusions leading to the recommendation for the conversion of the Panama lock canal to a sea-level canal, they have not backed up their discussions by scientific demonstration or by engineering and navigating experience. Just as broad judgments and an effort to penetrate the future are essential to an understanding of the place of the Panama Canal in the national economy and in the planning for national defense, so do scientific predictions of future traffic needs, the evaluation of weapon effects, and careful engineering appraisals of such problems as slides and navigation of the proposed sea-level canal also play a vital part.

For this reason the writer is particularly indebted to General Kramer for quoting the farseeing and prophetic instructions of Theodore Roosevelt, then President of the United States, to the Board of Consulting Engineers convened in 1905 to consider the type of canal to be constructed in Panama.[2] These instructions, and also the report of the Roosevelt Board and the conclusions of many other eminent investigators, not only established the "precept" (in the words of General Kramer) that guided the recently completed Panama Canal Studies; but, by test, proved to be a logical forecast that the decision to convert to sea level was ultimately inevitable when, for reasons of expediency, economy, and speed of construction, the lock-type canal was chosen. The Governor's recommendation to Congress under the provisions of Public Law No. 280, Seventy-ninth Congress, that the present Panama lock canal be converted at once to sea level, in effect implements a decision that was implicit in the acceptance of the lock canal in the first instance. In the light of experience during World War II and of recent weapon developments, the Governor's recommendation is as inescapable as it is timely.

Captain Nichols has had ample opportunity to evaluate the consideration given to the problems of navigating the proposed sea-level canal. In his discussion of the Symposium, he draws conclusions that should be reassuring to those having latent misgivings and should satisfy even the most skeptical that the proposed canal will be entirely adequate and will provide a safe and easy waterway for the navigation of ships.

Acceptance of the tidal computations for the Panama sea-level canal by General Pillsbury and Professor Bakhmeteff strengthens the case for the use of the computation method in evaluating conditions that would obtain in waterways of this kind. It is the view of the writer that for future waterway projects the large expense of model studies may well be avoided where (1) the extreme tidal conditions are reasonably predictable, (2) the channel cross

[137a] 29 W. 39th Street, New York 18, N. Y.

[2] "Report of the Board of Consulting Engineers for the Panama Canal, 1906," U .S Govt. Printing Office, Washington, D. C., 1906.

section can be made adequate dimensionally and of reasonable uniformity, and (3) the computed currents are found to be lower than those now encountered in the Cape Cod Canal (Massachusetts)—that is, of the order that may be expected in the proposed Panama sea-level canal.

It is hoped that the extensive investigations in the sea-level canal model suggested by Professor Bakhmeteff can be undertaken. Steps have been taken to protect and preserve the model in good condition for just such purposes.

Professor Davidson's interesting analysis gives clarification to the descriptions in the Symposium; and his acceptance of the model experiments related to the handling of ships in restricted channels adds authority to the testing method. Professor Davidson notes that in this testing the model operator (pilot in this case) is of vital importance to the effective operation of the model. The writer was impressed by his unqualified statement, when he deduces that perfect equilibrium may be difficult to attain, that: "* * * approximation to equilibrium is good enough in practice * * *." Professor Davidson's analysis of the model results by application to an actual sailing problem provides a clearer understanding than can be gained from an examination of the test results expressed in mathematical tabulations and graphs. Professor Davidson regrets that Messrs. Lee and Bowers did not emphasize the practical conclusions more strongly. This omission is the unfortunate result of the limitations in scope of treatment agreed on by the several authors to insure the fullest possible coverage in the several Symposium papers. Messrs. Lee and Bowers in the course of the investigations expressed full confidence in the practical application of the test results as did the Panama Canal pilots who were familiar with the Carderock (Md.) tests.

Messrs. Justin, Fucik, Hill, and Casagrande have contributed interesting sidelights on the Cucaracha formation, which is the most treacherous that will be encountered in the excavation for the sea-level canal. Mr. Fucik's analysis of the safety factor suggests that certain chances might well be taken with the Cucaracha to limit the volume of initial excavation with the expectancy that sloughing and slow moving slides, although contributing to the maintenance problem, may not entail total quantities approaching those that would be excavated initially if a high factor of safety is selected. A rather complete analysis by Professor Casagrande of what may happen subsequent to the completion of the sea-level canal makes it clear that a slow movement of the Cucaracha toward the canal cut may be expected in any event. This was the view of the staff employed on this phase of the investigations and was the consensus of the consultants. None felt, however, that there was a risk to shipping involved, since movements would be very slow and the quantities involved relatively small. Thus the material could be readily excavated as it moved into the canal. Because of the great depth and width of the proposed canal there would not be interference with traffic as a result of such movement of material or in the operation of equipment in its removal.

Mr. Hill indicates that the method of analysis employed in establishing the slopes of the Cucaracha is reasonably reliable. There is a seeming point of difference between Mr. Fucik and Professor Casagrande in that the former takes the view that the proposed slopes may be conservative and the latter the view

that they may not actually be conservative enough. Those are really expressions of the same opinion with respect to the future behavior of the Cucaracha from two different viewpoints—presumably Mr. Fucik's is related to that of probable ultimate cost whereas Professor Casagrande's is related to the quantity of Cucaracha material that may ultimately have to be removed. Both seem to agree that there will be some movement of the Cucaracha, and both are fully aware of the need for further laboratory and field investigations, for which plans have been suggested in the Governor's report. Fortunately the time available from the start of construction until the new cuts are opened to traffic will permit extensive tests and field observations of movements in the Cucaracha if any should occur. Whether the safety factor can be reduced is a design consideration; a reduction undoubtedly would not be undertaken unless the investigations in the course of design and construction warrant such action.

The admonition by Mr. Cedergren that "* * * new terrain must be examined closely for indications of old slides, and obviously unstable terrain avoided whenever possible" is one that will surely guide those who undertake the design and construction of the proposed sea-level canal.

It is believed that the estimates of cost provide fully for the difficulties in the dry excavation and in the disposal of materials by scows foreseen by Mr. McAlpine. The excavation and the handling of large volumes of material will both stimulate and support improvements of equipment and of method as construction progresses, but for reasons of conservatism of estimates this possibility has been discounted to provide against unforeseen contingencies.

General Covell presents an interesting analysis of the savings that would have resulted had the Panama sea-level canal been constructed as recommended by the majority of the Board of Consulting Engineers in 1906. General Covell's presentation of the views of both the majority and minority of this board in its report to President Theodore Roosevelt rounds out the discussion by General Kramer and shows that the final acceptance of the lock canal because of ease and speed of construction in no way constituted a condemnation of a sea-level canal in Panama.

Professor Hobbs' design for a sea-level canal at Tehuantepec (Mexico) completely overrides the conclusions as to channel requirements established by the investigations and tests under Public Law No. 280 and presented in the Symposium, and ignores minimum acceptable hydraulic standards for constricted ship channels. A one-way channel 175 ft wide which he proposes cannot be reconciled with a lock, 200 ft in minimum width, established by the United States Department of the Navy. The excavation involved in the canal suggested by Professor Hobbs is given as approximately 400,000,000 cu yd less than the proposed Panama sea-level canal. For a sea-level canal with adequate canal cross sections on the Hobbs alinement, the quantity of excavation would greatly exceed that required for the Panama sea-level canal.

A one-way tunnel, 63 miles long, would be a serious bottleneck to traffic. Towing locomotives would be required (for large ships at least) in the tunnel section and in the proposed single-way open channels. Under the most favorable conditions, almost 2 days would be required for the transit of such a canal; delays waiting to transit the tunnel would heavily penalize shipping. The

expense of installation and operation of towing equipment is apparently overlooked by Professor Hobbs, as is the fact that a new canal at any place other than Panama would require duplication of defense facilities and of administrative facilities, housing, shops, harbors, dry docks, etc., at a cost that may be expected to exceed $1,000,000,000.

The defense agencies take the view that a ship tunnel could be closed by demolitions despite the portal construction of the type proposed by Professor Hobbs. Forced ventilation for the disposal of stack gases and lining of the tunnel to prevent the falling of spalls or large blocks of rock on passing ships apparently are considered unnecessary by Professor Hobbs. If a ship tunnel were acceptable, the San Blas route would be much more favorable than the Tehuantepec route recommended by Professor Hobbs.

The reduction in travel time using the Tehuantepec route, where not offset by delays in transit, would apply principally to intercoastal shipping which is about 30% of the total traffic. For many trade routes the port-to-port travel distance via a Tehuantepec canal would actually be increased. The net annual saving in ship operating costs, if capitalized, would not justify the selection of the more costly Tehuantepec route.

In his list of advantages Professor Hobbs concludes that the combination of the existing lock canal and the Tehuantepec sea-level canal would provide the highest degree of security. Mr. Jarvis has somewhat the same view with respect to the combination of the Panama parallel sea-level canal and the present lock canal. Because of its vulnerability the lock canal would contribute nothing to security; and it would not be required to supplement the capacity of a sea-level canal. Therefore, the lock canal should be scrapped, rather than continued on either an operating or a stand-by basis at high expense. It is not apparent to the writer in what sense or degree the Tehuantepec canal would be less vulnerable to an attack than the proposed Panama sea-level canal as contended by Professor Hobbs. Rather, the reverse would seem to be true because of the highly vulnerable tunnel and tunnel portals and the narrow channels and because of its greater target length.

Mr. Dohn proposes that the canal be justified as an economic enterprise and suggests: (1) Increase of tolls and (2) decrease in canal construction costs. An increase in tolls in terms of the cost thus to be amortized would drive shipping away from the canal. His desirable objective of decreasing construction costs will undoubtedly be sought by those who undertake the canal construction. No one intimately familiar with the geologic formations and other local conditions would propose the use of the hydraulic giants for the excavation, or the development of power to be disposed of subsequent to construction.

Mr. Romanowitz voices the conclusion that rock excavation using bucket-ladder dredges would be both practicable and economical. He demonstrates that, since the employment of this type of dredge in the construction of the present canal, there have been major improvements which would reduce, very materially, lay-up time for repairs. He notes that the dredge *Corozal* did a very effective job of rock removal in the Panama Canal construction during its hours of operation and that only plant breakdown adversely affected its over-all usefulness.

Mr. Massey notes that no mention is made of the advisability of excavating to a depth of 130 ft with special dredges operating on Gatun Lake and, after lowering of the lake, of excavating to final project depth (15 ft of material). This procedure was considered but was left for decision in the course of actual construction. The bucket-ladder dredge will be tested fully before commitment is made to perform all deep rock dredging with this type of equipment.

Mr. Erickson suggests the use of a 60-in. hydraulic dredge for excavating the softer rocks and shales without blasting. Unfortunately a major part of the deep excavation in rock will require blasting. Nevertheless, in the design period consideration should be given to the size of the dredges, speed of cutter, etc., as he suggests.

Mr. Kazmann questions the advisability of expending large sums of money on the proposed sea-level canal and makes comparisons with such projects as the Saint Lawrence Seaway, the Missouri Basin development, and the Tennessee Valley Authority to the detriment of the proposed canal in its relation to national existence in the event of conflict. In reaching this conclusion Mr. Kazmann assumes that the canal project on the one hand and the development of national resources on the other are mutually exclusive. The extension of this reasoning would condemn the appropriation of funds for the further development of the atomic bomb and the development of rockets and improved airplanes and, in fact, for anything pertaining to the national defense. In any event a comparison of the canal project with other development projects was beyond the purview of the report, and the determination of related priorities among such projects is a function of Congress.

It is regretted that the Symposium led Mr. Kazmann to the conclusion that the investigations under Public Law No. 280 hardly bothered with the point: "* * * is one [a sea-level canal] needed?" Space limitations and security considerations may have resulted in an inadequate demonstration of the essentiality of the sea-level canal to the national defense and to the well-being of the American people; but the study nevertheless went into this problem exhaustively. From an examination of the Governor's report, the National Defense Establishment including the Joint Chiefs of Staff and the Department of the Navy, which has a special interest in the problem, fully supported and endorsed the need for a Panama sea-level canal. There is ample engineering proof contained in the classified studies for this conclusion. Professor Edwards notes that "* * * the question regarding the type of canal could be answered without such detailed technical studies." To the writer it also appears that by taking thought even a layman could not fail, from a knowledge of the features of the two types of canals and of present-day weapons gathered from the popular press, to arrive at the same conclusion.

Mr. Sheldon makes some pointed observations on the foundations of structures on Atlantic muck and warns against slides in this material after the lake has been lowered as the result of overloading the canal banks with spoil material. The setbacks from the canal slopes in the Atlantic muck to the edges of the spoil banks established in the studies are tentative; in the final design these and the rate and manner of disposal of spoil material should be carefully reviewed. Mr. Sheldon concludes that none of the possibilities of

failure which he describes constitutes a serious threat to the sea-level canal project; nevertheless, these possibilities should be anticipated and avoided in the designs.

The statements by Mr. Cole that—

> "* * * the investigation of the canal under Public Law No. 280 (Seventy-ninth Congress, First Session) was solely directed toward securing the authorization of the predetermined objective of a sea-level project on the premise that the present lock canal 'is totally lacking in security to meet the future needs of national defense,' and that the only alternative is a sea-level canal."

and

> "It is fair to ask the question, and insist upon a truthful and an unbiased answer: Is an expenditure of $2,500,000,000 justified at Panama or elsewhere on the Isthmus for any canal, lock or sea level, under any circumstances?"

and

> "The estimated first cost for 'converting' the lock canal to sea level, of $2,483,000,000 alone (which appears to be grossly underestimated), is prohibitive, unless the cost is to be borne by the national defense budget."

—have the effect of impugning the integrity of the organization employed on the studies under Public Law No. 280—the Governor of the Panama Canal, the Board of Consulting Engineers who fully concurred in the conclusions and recommendations reached in the studies, and the authorities of the National Defense Establishment, including the Navy General Board and the Joint Chiefs of Staff, and also the Atomic Energy Commission in its evaluation of atomic bomb effects. Heretofore the writer has been disinclined to reply to such charges; but, since they have now reached the Society's publications, the writer challenges them and expresses his resentment of these implications. Honest differences of opinion are certain to develop on many features of the canal problem, now as in the past, but they should be expressed without the invidious implications used by Mr. Cole.

Mr. Cole's charge that Congress and the government officials had "the predetermined objective of a sea-level project" is best answered by the record of Congressional hearings and debate on the bill which became Public Law No. 280.

Mr. Cole and Captain Little both note that the investigation reported in the Symposium was not directed toward securing any major improvement in the operating deficiencies of the present lock canal. In Public Law No. 280, Congress explicitly directed a

> "* * * comprehensive review and study of the means of increasing the capacity and security of the Panama Canal to meet the future needs of interoceanic commerce and national defense * * *."

The exclusive pursuit of other secondary objectives would have been in violation of the realistic and comprehensive directive of Congress. It was considered that the factor of the operating deficiencies of the present canal was a con-

comitant phase of the studies and this was looked into fully. It is unfortunate that the details of the studies could not be presented within the scope of the Symposium; nevertheless, the designs of the lock canals studied incorporated features to correct the deficiencies noted by Mr. Cole in his references. These are fully set forth in the Governor's report.

Lest it be gathered from Mr. Cole's discussion that the present canal is unduly hazardous to shipping, it should be noted that the accident records of all the major waterways throughout the world were carefully examined and it was found that the Panama Canal is as safe, from an operating viewpoint, as any other major waterway. Damage to ships in more than 200,000 transits through the Panama Canal since it was placed in service has required repairs at an average cost of $7.45 per ship transit. Truly this is a notable record and a complete answer to those who have dwelt unduly on the operating deficiencies of the present canal. The facts are well recognized by marine insurance underwriters, for they attach no special risk to the Panama Canal.

Mr. Cole does not identify the authorities who have expressed the opinion that any type of canal is definitely vulnerable to the atomic bomb. To weigh any such statement, the damage must be related to the period of closure that would result from atomic bombing. In the case of a sea-level canal this would be a matter of weeks at most. In the case of a lock canal it would be a matter of years, for if Gatun Lake were lost it would take 18 months to recapture sufficient water to make the transit of the canal possible and to this there must be added the necessary period for repairs and reconstruction to make impoundment possible. It is not understood, as contended by Mr. Cole, how "* * * the major premise (national defense), on which the recommendation for 'converting' the canal to sea level was based, has now definitely been removed"; or how he reconciles this conclusion with that of the National Defense Establishment that only a sea-level canal will serve the future needs of national defense. It would be interesting to have an evaluation of the effects of conventional bombing on both the sea-level and the lock-type canals, by the authorities to whom Mr. Cole alludes.

Mr. Kirkpatrick evidently holds with Mr. Cole that the solution of the operating problems of the present canal will provide for the future needs of commerce and the national defense, thus satisfying the requirements of Public Law No. 280, Seventy-ninth Congress.

Mr. Kirkpatrick's warning that: "* * * going down 100 ft further [below the present canal bottom] * * * is daringly unprecedented even to the point of recklessness" suggests that had the Panama Canal constructors taken counsel of such fears they would have abandoned the undertaking, for they too had nothing to go on but, to use Mr. Kirkpatrick's words, "geological conjectures." Their conjectures, moreover, were on a much sketchier basis than are those of today, for out of their great accomplishment there is an accumulation of knowledge and of experience which, supported by present-day techniques of exploration, test, and stability analyses gives assurance that none of the dire events which apparently haunt Mr. Kirkpatrick will happen.

Inspection by the Panama Canal engineering staff of certain mining operations disclosed that the bucket-ladder-type dredge is capable of lifting

boulders and rocks very much larger than the buckets. This is also true of clamshell and dipper dredges, but one could scarcely contend that the excavating of oversize rocks would not interfere with the over-all plant efficiency of any of these excavators. In making his point that the bucket-ladder-type dredge would not be efficient in the removal of large boulders or hard rock, Mr. Erickson indirectly emphasizes the importance of adequate drilling and blasting.

Mr. Jarvis is of the opinion that tidal-regulating works are unnecessary. His studies of the Cape Cod Canal and actual operating experience there give authority to his conclusion. There is considerable merit to the Panama parallel sea-level canal, as pointed out by Mr. Jarvis. However, it was the conclusion of the Board of Consulting Engineers, as well as of the Panama Canal authorities, that the advantages did not outweigh the additional estimated cost of $800,000,000 for a sea-level canal on the Panama parallel route. Moreover, since a single sea-level canal would fulfil the future needs of commerce and national defense there would be no justification for subsidizing, in addition, a lock canal that could be rendered useless even by light bombing.

Much has been written (as Captain Little remarks) about the capacity and the time of transit of the canal; and, contrary to Captain Little's observation, a great deal of factual material bearing on these quantities is published in the Symposium and more in the Governor's report. All this has been done in an effort to arrive at facts, without creation of any impression, exaggerated or minimized, contrary to the truth.

Captain Little is apparently in disagreement with the use of the term "capacity" as defined and employed in the Symposium. The canal, of course, could transit the number of ships computed by Captain Little, if these ships reached the canal at the unvarying rate of 36 a day—that is, if shipowners would tolerate delays of the necessary, and in individual cases uncertain, amounts of time required to realize such an assumption. If one were to take for illustration the capacity of 13,140 ships per yr (designated by Captain Little) (36 ships $\times$ 365 days) one would find from Table 7 that the abnormalties of traffic flow that would obtain would result in 495 ship-days of delay when traffic is 130% of the daily average; on 140% peak days, 336 ship-days delay; and, on 150% peak days, 162 ship-days delay. Using ship-operating cost data furnished by the Maritime Commission, the delay penalties inflicted on the shipping industry for such peak days only would exceed $3,000,000 annually. For all delays, taking into account every day on which shipping would be delayed because the number of arrivals for transit would exceed the daily average of 36 ships, the total annual penalty to shipping would be very much higher.

Captain Little evidently does not take exception to the indisputable fact that there is a wide daily fluctuation in traffic arriving for transit of the Panama Canal (equally true of Cape Cod, Suez (Egypt), and all other canals), for he proposes regulation of traffic to avoid peak congestion; yet he dismisses this factor in his computation of the capacity rating of the Panama Canal by assuming that the daily fluctuations can be easily ironed out. This same problem, the cost imposed by peak loads, plagues many of man's activities,

as engineers are well aware. The electric and rapid transit industries would be grateful for a practicable solution of it. They have not been able as yet, in Captain Little's triumphant phrase, to "control them [peak loads]!"

It would be interesting to have a canvass of the shipping industry to ascertain its views on Captain Little's proposal of "controlling sailings to, or arrivals at, Panama." The evident procedure for establishing such control is: Speed up loadings or delay ships in loading ports or en route to avoid the peak traffic condition. An assumption as to the workability of the former implies that shippers now generally conduct loadings inefficiently, and thus could at will improve loading rates to lessen time in port to fit traffic call-up schedules for transiting the canal. Does Captain Little truly believe that shippers will willingly accept delays (and the costs that go with delays) to meet man-imposed regulations to overcome canal operating deficiencies that are susceptible of correction by engineering methods?

In taking exception to the significance given to "transit time" in the studies described in the Symposium, Captain Little states: "No general or admiral ever had to 'tear his hair out' because some ship or convoy of ships was late in passing through the Panama Canal." This is the finest compliment that could be paid the existing canal. It was, in part, to insure for the future this same quality of service that Congress directed the studies under Public Law No. 280, Seventy-ninth Congress. The report of the Governor is fully responsive to the Congressional directive and his recommendation meets this objective and also the additional one imposed by Congress, to wit—that of insuring that an enemy cannot seriously interfere with the capacity performance of the canal, no matter what weapon is employed. By using military operations to analyze transit time Captain Little gives only a part of the picture. In peacetime the Navy traffic through the canal is less than 15% of the total. Commercial traffic, which constitutes the remainder, would most certainly welcome reduction in transit time and resulting savings.

The answer to Captain Little's objections to the conclusions reached in the studies with respect to the vulnerability of the proposed sea-level canal is given in the writer's paper and elsewhere in this closing discussion. Specifically it is stated in the writer's paper (under the heading, "Security of the Panama Sea-Level Canal") that:

> "The tidal-regulating and flood-control structures of the sea-level canal would not be essential to its safe operation; hence their damage or destruction would result only in temporary interruptions to traffic. * * * The flood-impounding structures being of earth and rock construction would be highly resistant to bombing but, because they are not absolutely vital to the operation of the sea-level canal, the costly treatment necessary to make them resistant to the atomic bomb would not be warranted. If damaged by bombing, the flood-control structures could be readily repaired and restored to service."

These conclusions were the product of an unbiased study by competent experts, and were formally accepted by the Secretary of the Navy, the Navy General Board, the Joint Chiefs of Staff, and the Atomic Energy Commission. The details of the studies are classified and cannot be published. It goes without

saying that Congress, in its hearings on the Governor's report prior to the enactment of legislation, will have such access as it desires to the classified studies and can satisfy itself that the conclusions are tenable.

The political aspect of the problem of conversion posed by Captain Little will receive the attention of Congress in its hearings. The Governor's report in dealing with the problem raised by Captain Little states:

> "Arrangements would have to be made with the Republic of Panama and compensation made to landowners for the right to inundate an additional area of about 10,000 acres downstream from Madden Dam. The completion of the sea-level canal, however, would permit the return to Panamanian jurisdiction of lands now inundated by Gatun Lake and having an aggregate area greater than the additional area on which flowage rights would be needed."

Except for such land exchange the present treaties with Panama are adequate to cover conversion of the canal within the Canal Zone.

In using the words, "* * * it was a foregone conclusion that no scheme other than a sea-level canal at Panama would receive any real consideration under the act" Captain Little takes the position with Mr. Cole that some preconceived conviction guided those responsible for the studies. The answer given to Mr. Cole is extended to Captain Little.

The color of language employed by the writer in stating that with the approach of war the restricting effect of the small locks became intolerable on the design of Navy ships seems to have bothered Captain Little. The literal fact is that the 140-ft width of the proposed Third Locks, and the 200-ft width of locks used in the present studies, were established by the Navy. Captain Little assumes that the canal authorities established these widths from "guarded replies" by the Navy to specific inquiries. This is pure conjecture. His leading question on the frequency of the use of the canal by large carriers or oversize battleships should be addressed to the Department of the Navy.

Captain Little takes exception to the writer's statement on the navigational safety of the sea-level canal and implies that fogs, heavy rains, and machinery failures were given inadequate consideration. All that can be said is that these matters were studied, it is believed adequately, and that the published report gives the data used and the conclusions reached from study of similar canals and from the advice of the pilots and various naval and maritime authorities.

The capacity of the Panama sea-level canal would not be 69 ships as stated by Captain Little, but under the conditions stated in Tables 12 and 13 would be much greater.

Captain Little suggests that for comparison with the proposed Panama sea-level canal there should be a lock canal with a Pacific terminal lake and without any protective features. Such a plan with locks 200 ft wide was investigated. It would cost $1,600,000,000 approximately. A reduction in lock dimensions to provide "reasonable lock sizes," as suggested by Captain Little, would decrease the cost of this plan in proportion to the dimensions determined by the Navy to be acceptable.

Rear-Admiral D. E. Barbey's views[131] cited by Captain Little seem to be at variance with the official views of the Department of the Navy. Of the 17,000 vessels which transited the canal in war years (3½ years approximately), 30% were naval fighting craft. This fact would seem to refute the statement that: "* * * the Panama Canal has become * * * a secondary route for critical raw materials in time of war." It is devoutly to be hoped that the enemy will subscribe to Admiral Barbey's view as quoted and will not attack the canal. Congress had in mind that the enemy might act differently and in effect asked the question: "What if he does?" It may be remarked that a canal easy to destroy would be a much more tempting target than a canal on which the same effort could inflict but minor damage and so the first might be regarded as a profitable target whereas the second might not.

In discussing target values Gen. Leslie R. Groves apparently did not state that the Panama Canal would be an unprofitable target as implied by Captain Little. No contention is made in the report that the enemy would find the Panama Canal a more profitable target than New York, N. Y., or any other industrial center. The answer to such a question can only be an indirect one: Is the Panama Canal essential to national defense? The National Defense Establishment has found that it is.

The Governor's report and the Symposium have very properly emphasized the urgent need for a sea-level canal to meet the future needs of national defense. As described in the Symposium the study of future traffic needs revealed that delays to ships awaiting transit will be experienced on "peak traffic days" beginning about 1960. After 1961 the cost of delays at the entrances to the canal will increase in direct relationship to the growth of traffic. In 1970 they will total more than $3,000,000 unless remedial measures to increase canal capacity have been undertaken. After 1970 there will be a sharp rise in the penalties arising from inadequate capacity averaging $20,000,000 until 1980 when the added cost will be more than $70,000,000 annually. The heavy financial penalties resulting from the failure to provide against the deficiencies in canal capacity which will first be felt in 1960 will fall upon shipping interests who must either accept delays or, when delays become intolerable, route their ships around Cape Horn.

The cheapest method of increasing the capacity of the canal to meet the needs of commercial shipping would entail certain modifications to the locks of the present canal at a cost of approximately $130,000,000. These corrective measures, however, would not add to the security of the canal or in any way reduce the risk of loss of the canal by enemy action. Since the penalties to shipping as a result of inadequate capacity will soon become sufficiently oppressive to demand a large expenditure of public funds, prudent planning dictates that the remedial measures provide both for future traffic needs and for the requirements of national defense. The only answer is early conversion to a sea-level canal.

If the lock canal is ever seriously damaged by enemy action, and particularly if the damage inflicted results in the drainage of Gatun Lake, as would be the

[131] "Can We Defend the Panama Canal?" by Sidney Shalett, *Saturday Evening Post*, October 9, 1948, p. 162.

case with even minor bombing of the lock gates, not even its most earnest proponents could advocate its reconstruction as a lock canal. A successful attack on the canal would mean a denial of the Isthmian crossing to shipping for the 10-year period required for the construction of the sea-level canal, unless traffic could be restored as construction progresses. The loss of the canal prior to 1960 would increase shipping costs as the result of lengthening of trade routes around Cape Horn an average of $400,000,000 per yr. After 1960 the increased cost per year would be $500,000,000 and would rise to $600,000,000 per yr after 1980. Thus, unless, through some expediency, early restoration of traffic in the damaged lock canal proves practicable, the increase in commercial ship-operating costs alone in the construction period would total approximately twice the estimated cost of peacetime conversion to sea level. The disruptive effects of the loss of the present canal on the conduct of war combined with the penalties that would devolve upon shipping in the postwar construction period are sufficiently alarming to warrant its conversion at once.

If the lock canal is ever badly damaged, every effort would be made immediately to restore traffic in the lock canal if this is practicable. The first step in any case would be the restoration of the summit lake (Gatun Lake) which in itself would greatly facilitate the conversion to sea level. Whether it would be possible, again, to place the locks in operating condition after enemy attack would depend on the extent of damage inflicted. Atomic bombing of the locks would require their abandonment.

Even though restoration of traffic through the lock canal is found practicable, it would probably be on a restricted basis and a considerable part of postwar shipping would have to be diverted around Cape Horn. Moreover, if the sea-level conversion is deferred until the lock canal is crippled by enemy action, the cost of the conversion would be materially increased. The increase would result from the cost of the temporary repairs to the lock canal, from the inevitable lengthening of the construction period, and from the added difficulties during construction if the lock canal is not fully and immediately available for utilization in effecting the conversion to a sea-level canal.

Dean Marston's references to the studies by the late Sydney B. Williamson, M. ASCE, and those of the Interoceanic Canal Board lead the writer to acknowledge that much dependence was placed on these studies in the recent investigations under Public Law No. 280. Dean Marston's acceptance of the navigable pass and his conclusion that the canal would be navigable even if the tidal locks should be destroyed are noted with special interest as are his views on the studies made to avoid slides in the proposed sea-level canal. His appreciation of the fact that the present canal has been a full-scale laboratory for the study of safe slopes high lights a consideration on which major reliance has been placed in establishing the slopes for the sea-level canal.

RALPH P. JOHNSON,[138] M. ASCE, AND SYDNEY O. STEINBORN,[139] ASSOC. M. ASCE.—Captain Nichols has stated that use of the estimate of 1.5 ships per

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[139] Asst. Chf., Civ. Works Planning Branch, Seattle Dist., Corps of Engrs., Seattle, Wash.; formerly Engr. in Chg. of Traffic Studies, Special Eng. Div., The Panama Canal, Diablo Heights, Canal Zone.

lockage through the present locks may be overly optimistic for planning purposes. It is believed, however, that a brief elaboration of the derivation of this estimate will establish its essential conservatism.

During the past 20 years, the average number of ships per lockage has been about 1.2. However, the canal has not had to operate near its capacity and a greater percentage of tandem lockages would have been possible if required to meet traffic demands. Assuming a group of 100 vessels of the length composition expected in the year 2000, the existing lock chambers could accept lockages composed as follows:

Description	Lockages	Ships
All ships longer than 500 ft, singly	11	11
One ship of the 400-ft to 499-ft group with one below 399 ft	44	88
Remaining 400-ft to 499-ft ship, alone	1	1
Total	56	100

(In Table 9, vessels are estimated to increase in length; this has the effect of reducing capacity and the assumption is thus conservative.) In this manner, an average of 1.8 ships could be transited per lockage. The soundness of this determination was established by routing actual traffic on peak days of record through the canal without delaying any ship more than 2 hours. In the further interests of conservatism, however, additional consideration was given to delays that might be caused by attempting to compose lockages according to the foregoing tabulation; for this reason the 1.8 factor was reduced to 1.5 for planning purposes.

Captain Nichols questions the application of the 1.5 factor, which was determined on an annual basis, to daily traffic. This question also arose during the course of traffic and capacity studies and a comparison of peak-day traffic and annual traffic (in average percentages) was made as follows:

Length of ship, in feet	22 peak days in 1933 and 1939	1921–1940
Over 500	6.1	6.2
400 to 499	49.0	47.3
Under 400	44.9	46.5
Total	100.0	100.0

This comparison made it evident that, historically, there was little difference between the length composition of traffic on peak days and the length composition as averaged over many years. Thus, it was concluded that an estimate of 1.5 ships per lockage was conservative for both annual and daily traffic. It is acknowledged, however, that traffic abnormalities will occur and there may very well be days when few tandem lockages can be made. Fortunately, the occurrence of such days will be rare and thus they have little design significance.

Captain Little has stated that "* * * very little in the way of factual analysis has been published on the actual 'capacity' of the Panama Canal * * *." It was the intention of the writers to correct this situation in preparing the section of their paper under the heading, "Capacity of the Present Panama

Canal." In that section were given the results of time and motion studies which established the capacity of the locks, and which described the effect of fogs, one-way night traffic, and "clear-cut" transits. Similar treatment was given to the capacity of the locks during the overhaul period and finally, as a convenience to the reader, the entire matter was summarized in Table 10. The conclusions thus summarized were the results of months of intensive analysis of lock operations, in which the researching engineers consulted frequently with the experienced lock operating personnel of the Panama Canal. Whether or not the capacities shown are "actual capacities" will not be evidenced until the demand for transits requires that operations conform more or less closely to the procedures and timing evolved in the course of this investigation.

It is certain, however, that, in referring to capacity, the writers are not dealing with annual capacity. The capability of the Panama Canal is similar in some ways to the peaking capability of a hydroelectric power system in that it is established by its performance during a critical period. In this particular instance, the critical period occurs during the 4-month biennial lock overhaul. Its annual capacity has little practical significance because it is manifestly impossible to expect shipping interests to wait until after the 4-month overhaul period to be assured that their cargoes will move without being delayed in Canal Zone waters.

It is equally impracticable to speak of controlling shipments during the overhaul period in order that peak traffic days may never materialize. It is impossible to conceive any amount of planning, short of that which might be possible in a world totalitarian state, that would prevent traffic peaks at the Panama Canal. It should not be forgotten, however, that rigid traffic control does exist in Canal Zone waters. Whenever the dispatchers are confronted with a backlog of ships awaiting transit, scheduling is accomplished with considerable skill and efficiency, the limiting features of the transiting program being set by the physical plant of the waterway.

Captain Little's inferred recommendation that traffic be controlled by increased tolls for transits during peak traffic periods is completely unacceptable when applied to a public utility like the Panama Canal. Faced with increased traffic demands the canal must provide additional facilities. The alternative is a reduction in service which is not acceptable to the shipping industry. This fact has been brought out by Captain Nichols in his discussion:

> "Upon release of the biennial lock overhaul schedule for the winter of 1947–1948, several local steamship agents inquired as to the probability of delays to shipping. They stated that their principals, engaged in shipping between the Antipodes and Europe, had intimated that delays would result in rerouting via the Straits of Magellan or the Suez Canal."

The Panama Canal has a fine record of efficient public service; it was built as an asset to national defense and as an aid to world trade. Its defense has now become a major problem and its inadequate capacity will soon begin to complicate world trade. Therefore, it is time to think about modifications that will solve the defense problems and that will permit the canal to continue its long history of accelerating the movement of world cargoes.

F. S. BROWN,[140] M. ASCE.—The writer concurs with Mr. Edwards that the present Gatun spillway does not have the capacity to handle the "spillway design flood." However, the statement questioned by Mr. Edwards that "The control afforded by Madden Lake has largely ended the likelihood of further major flood disturbances in the present canal" was not intended to refer to this deficiency, but rather to the benefit afforded by the Madden Lake control in reducing objectionable crosscurrents in the canal at Gamboa at the mouth of the Upper Chagres River.

J. S. MEYERS[141] AND E. A. SCHULTZ,[142] ASSOC. MEMBERS, ASCE.—The serious interest evinced by the discussers of this paper is gratifying. Because of the diverse nature of the tidal-current problem, the discussers have generally been concerned only with certain features, and it is considered best to reply separately to each one.

General Pillsbury's discussion of the precision and the reliability of velocity computations is a welcome amplification of the condensed explanation made in the basic paper. Professor Bakhmeteff's discussion, written in similar vein but directed toward a more general appraisal of the entire tidal-hydraulic problem, is also a very welcome addition to the briefer summaries in the original paper.

Professor Reynolds comments on the scanty knowledge of channel roughness coefficients for large channels, and questions whether the conclusions which were drawn from available data (conclusions (*a*) and (*b*) under the heading, "Hydraulic Roughness of Large Channels: Roughness Data on Large Channels from Other Sources") really indicate the true situation. The writers agree wholeheartedly that more data need to be measured and studied, which may well lead to different conclusions. The conclusions as expressed, however, are quite well supported by the information that was collected.

Interest was expressed also in the roughness measurements made in Gaillard Cut. The uncertainties inherent in such observations, as indicated by Professor Reynolds, were recognized and overcome as far as possible. It was a source of some surprise to the writers that the measurements were so consistent and successful, and that the anticipated difficulties did not prove to be serious. The measurements of velocities and of cross sections posed no difficult problems, but the determination of the very small differences in water level was approached with great trepidation. Fortunately the weather was ideal for the purpose on all three nights when observations were made, and careful and intelligent observers were secured. There was absolutely no discernible wind from sundown to sunrise; the water had the glassy appearance characteristic of perfect calm; and barometric pressures were steady.

Surges created by operation of the lock valves persisted, with diminishing intensity, throughout the tests. Consistent surge patterns were observed at each of the hook gages; and, after the lapse of a few hours, the difference in

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level between trough and crest of a surge dropped to approximately 0.010 ft. Average water levels used were those taken over an 86-min period—the time required for a full surge cycle.[143] The roughness coefficients computed from the measured velocities and differences in water level were remarkably consistent, in spite of the very flat slopes and the considerable irregularity in cross-sectional area for successive reaches. The results are firmly believed to be reliable, and not just the product of a series of fortunate agreements.

The statement (under the heading, "Model of Sea-Level Canal: Model Scale") in the paper that the resistance coefficient for the model "* * * was found to correspond to the selected Manning coefficient of 0.024 * * *" should have been better explained. Actually, the roughness coefficient for the concrete surface of the model was 0.011 when computed for the measured model velocity (Fig. 33(*a*)) of 0.37 knot, in the model channel 0.6 ft deep and about 6 ft wide. When the same observed slope and the prototype values of 3.7 knots, 60-ft depth, etc., were used in the Manning formula, the roughness value for the corresponding full-sized channel worked out as 0.024. Different sets of model observations, for the different test channels and for steady flow in both directions, gave model roughness coefficients varying from 0.011 to 0.012, corresponding to prototype coefficients from 0.024 to 0.026.

The question was raised as to the reliability of water-level measurement in the model by recording gages and hook gages, in contrast to the measurements obtainable by point gages. Actually, point gages mounted directly over the model channel were used in a number of tests as a check on the indications of the other gages. It was found that hook gages, on which long series of successive readings could be made more quickly and easily, also gave more consistent and accurate results. By the use of small stilling cans and connecting pipes of ample size and the shortest possible length, water levels in the hook-gage wells were made to follow those in the main channel without perceptible deviation. To insure reliable water-level data, the model was left filled with water after each day's operation, and all gage zeros were checked each morning from a still pool. This method, although it was never popular with the individual whose turn it was to reach the model at 4:00 a.m. in order to complete the series of zero readings before daybreak, was much quicker as well as more precise than the alternate method of spirit leveling. Machined reference marks on the metal posts supporting each hook gage and recorder, and an auxiliary hook gage in the stilling well of each recorder, made it easy to to keep account of the small variations in zero that did occur, and to make daily corrections when necessary.

The automatic recording gages were convenient for making adjustments and for trial runs; but, even after the original instruments were extensively modified and improved, they were less precise and dependable than the hook gages; so the latter were used for all record runs. Electric water-level indicators of several types, including the type developed for the Cape Cod model,[80] were considered for the Panama model. However, the comparatively gradual

[143] "Surges in Panama Canal Reproduced in Model," by F. W. Edwards and Edward Soucek, *Transactions*, ASCE, Vol. 110, 1945, p. 345.

[80] "Electrical Water-Level Control and Recording Equipment for Model of Cape Cod Canal," by H. L. Hazen, *Electrical Engineering*, February, 1937, p. 237.

changes in elevation, the smooth water levels in this model, and the problems involved in an outdoor installation for widely separated measurement points, as well as the probable delays in design and manufacture of such equipment, soon led to a decision against the electric gages.

Mr. Horton refers to the velocity estimate of 2.6 knots based on the Eytelwein formula. This estimate was made in 1924 by the United States Coast and Geodetic Survey at the request of the Panama Canal, and was included in the basic paper more as a point of interest than as a carefully considered prediction. The writers do not know the basis for the statement of the Coast and Geodetic Survey that the 2.6-knot estimate agreed with the observed values at Cape Cod. It should be remembered that the Cape Cod Canal channel was then a much shallower and narrower waterway, and the body of information developed during the investigations for the enlarged Cape Cod, Chesapeake and Delaware, and Panama canals did not then exist.

No computation of Cape Cod Canal velocities was made during the Panama investigation as a check on the validity of the computation methods. The writers agree fully with Mr. Horton, however, in the belief that the computation methods used for the Panama sea-level canal are equally applicable at Cape Cod.

Since the paper was prepared, several letters on the subject of tidal velocities were exchanged between the Panama Canal and Johan van Veen, chief engineer of the Rijkswaterstaat of the Netherlands, whose work has been referred to in the discussions.[105] Using methods developed for use in the many tidal canals in the Rhine lowlands, Mr. van Veen made an independent computation of velocities for a Panama sea-level canal.[144] His predicted velocities agreed closely throughout the tidal cycle with those computed by the Pillsbury method, and provided further reassurance of the reliability of the existing computation methods.

The description by Dean Allan of the steady-flow method for computing the limiting values for tidal velocities is a useful addition to the original paper. The general agreement between the steady-flow results and the Pillsbury results was very reassuring in the early stages of the Panama investigations, before other supporting evidence had been developed.

For the information of those who wish to make detailed comparisons of the computed steady-flow velocities (Fig. 113) with the velocities obtained at Panama (Figs. 35 and 36), it should be stated that the canal channels used in the two instances were not quite the same. Dean Allan used an effective channel length of 220,000 ft with uniform side slopes of 1 on 1, whereas the examples in the basic paper were for the finally recommended length of about 200,000 ft with side slopes varying from 1 on 2.5 to 3 on 2, as indicated in Fig. 30.

The steady-flow method is regarded by the writers as a desirable check on the maximum velocities estimated by other methods, but not as a satisfactory substitute for a more complete computation method. It was found, after computers became familiar with both procedures, that a Pillsbury computation

[105] "Analogy Between Tides and A. C. Electricity," by Johan van Veen, *The Engineer*, November 28, 1947, p. 498.

[144] "Estimates of Tidal Currents," by Johan van Veen, *Isthmian Canal Studies Memorandum No. 299*, Diablo Heights, Canal Zone, February 18, 1948.

did not take much more time than a steady-flow computation. The expenditure of "many thousand man-hours of computation," as stated by Mr. van Veen and cited by Dean Allan, was never needed at Panama, although such an amount of time might well be required for the highly complicated networks of canals with which Mr. van Veen was concerned in the Netherlands. As nearly as can now be recalled, it is believed that little more than 1,000 man-hours of computation time were expended for all the sets of velocity computations made by the Pillsbury method during the 2-year period of the investigation.

Mr. Massey's suggestion of the possibility of reducing average velocity by transverse submerged dikes is of interest. Tests made in 1933 at the United States Waterways Experiment Station[145] in Vicksburg, Miss., indicated that this idea would be helpful for the situation at the outlet of Lake Huron, where it was desired to raise the lake levels and thus produce deeper water in the harbors. The conditions for those tests were different from those that would be found in a sea-level canal at Panama, however, and the results of the earlier tests apparently should not be applied directly to the Panama situation.

Since the proposal for transverse sills would interfere with the passage of larger ships than those now in use, and particularly with emergency transits of crippled ships, it would probably not be acceptable from a navigation standpoint, but the effect of such sills on velocities could readily be determined by tests on the sea-level model. Restricted channel tests on the performance of ship models operating over such sills have already been recommended as a feature of a schedule of further investigations to be undertaken if a sea-level canal project is approved.

Professor McNown's discussion, showing the close correlation between the observed sinkage values for the ship models and the theoretical change of level as computed by the Bernoulli relationship, duplicates an analysis used for checking results during tests at the Taylor Model Basin. An explanation of this analysis was one of the items omitted from the Lee-Bowers paper because of space limitations, but it has been explained in the later Taylor Model Basin report[146] on the restricted channel tests. W. van Lammeren,[147] among others, has also worked on this subject and developed equations similar to Professor McNown's Eqs. 7 and 8. The comparisons between theoretical and observed sinkage on ship models as plotted in the Taylor Model Basin report are virtually identical with Fig. 117.

In 1947 sinkage was measured on four full-sized ships transiting the Panama Canal, for a comparison between full-scale observations and the theoretical values indicated by the Bernoulli relationship. Two measuring stations were used: One in Gaillard Cut where surface width was 370 ft, water depth was 46 ft, and cross-sectional area was about 15,000 sq ft; and one in the upper end of Gatun Lake where surface width was 1,100 ft, depth in the navigation channel

[145] "Experiments to Determine the Backwater Effects of Submerged Sills in the St. Clair River," U. S. Waterways Experiment Station, Vicksburg, Miss., April, 1934.

[146] "The Performance of Model Ships in Restricted Channels in Relation to the Design of a Ship Canal," by R. S. Garthune, B. Rosenberg, D. Cafiero, and C. R. Olson, *David W. Taylor Model Basin Report No. 601*, Washington, D. C., August, 1948.

[147] "Weerstand en Voortstuwing van Shepen" (Resistance and Propulsion of Ships), by W. van Lammeren, H. Stam, Holland, 1942.

was 46 ft, and cross-sectional area was about 35,000 sq ft. Ship speeds were from 6 knots to 7 knots past the first measuring station, and from 9 knots to 10 knots past the second. Measured sinkage for individual transits varied from 0.37 ft to 1.9 ft, whereas computed theoretical sinkage varied from 0.37 ft to 1.3 ft. In general, the computed sinkages were good approximations of the observed sinkages. The practical difficulties of measuring, accurately, the sinkage of a full-sized ship in normal operation, and the irregularities in the canal channel led to a greater spread between observed and computed values than was the case for the laboratory tests where all factors could be kept under better control, and any questionable observations could be repeated, but the validity of the analytical method was well demonstrated.

C. A. LEE[148] AND C. E. BOWERS,[149] JUNIORS, ASCE.—The discussions of this paper are greatly appreciated and the writers sincerely hope that their paper, clarified and enlarged on by these discussions, will form a base from which a clearer analysis of these troublesome problems may be presented.

Professor Davidson's objections, perhaps, need further clarification. Certainly there was never any intent on the part of the writers other than to show their appreciation for the counsel of the Panama Canal pilots. It is well understood that the experience of the pilots afforded great assistance to the personnel engaged in maneuvering tests and also aided in the decision to use approximately 5° of rudder angle as a basis for estimating the proper width and depth of the channel. It should also be emphasized that the selected rudder angle of 5° is a practical one and certainly does not account for extreme conditions of operation when the vessel is a considerable distance off center in the channel.

It is not clear to the writers how Professor Davidson proposes to use his analysis of moments, forces, and time to compute the most efficient width and depth of channel for a given ship speed. A minimum time factor would have to be established which would include time of reaction of the pilot, the rate of rudder movement, the rate of swing, and the speed of the ship. This operation would be very complex when the ship is entering or leaving a channel bend. A bend or a curve in a ship channel is considered one of the danger points due to the fact that the pilot must permit the ship to yaw in the channel and thus develop the aforementioned forces and moments. If the ship, on leaving a channel bend, is off center in the channel (which, in practice, is quite often the case), the turning of the ship must be retarded, the yaw corrected, and the vessel restored to its proper position in the channel. The time that the pilot has at his disposal is a function of the extent to which the ship is off center in the channel. It is believed by the writers that, although a time study might tend to clarify conditions somewhat, the variations imposed by a channel bend are so many and so complicated that no useful information could be obtained.

Should a time study be used for a straight channel design it would still have to be based on selected rudder angles. If a range of rudder angles and off-

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center positions were used to compute the time factors for several channel widths and depths and ship speeds, a question still remains as to how much elapsed time, or developed force, would be considered safe. It is believed that considerable disagreement would result if such a case were presented to a group of pilots. In discussing such matters with the pilots it was found that the rate of application of the rudder changed with the pilot, and thus the rate of swing of the ship would vary. Some pilots appeared to have a tendency to apply the rudder very rapidly whereas others applied the rudder gradually. As a matter of fact the speed of the rudder control mechanism differs from ship to ship. In terms of rudder angle this is a real value that immediately registers with the pilots and it is a term they all understand.

From Professor Davidson's discussion of forces and moments acting on the ship in an off-center position it is believed that the writers were not too clear in their description of Figs. 47 and 48. Professor Davidson states:

> "A rudder angle of 3.3° is sufficient to offset the moment, the water force on the rudder acting away from the bank. Since the rudder is about 350 ft aft of the center of gravity, the rudder force must then be about 400/350, or 1.14 tons. Fig. 47 shows, however, that in free running a rudder angle of 5° is in fact needed, corresponding presumably to a rudder force of 5/3.3 times 1.14, or 1.7 tons. The forces and moments then are not balanced, and so the ship must assume a small yaw angle."

The ship as represented by the data plotted in Fig. 47 is in equilibrium, and the ship as represented by the data plotted in Fig. 48 is not in equilibrium. Fig. 47 represents a condition for which all the forces acting on the ship are in balance, and the center of gravity of the ship will travel parallel to, and at a fixed distance from, the channel bank. Fig. 48 represents a condition under which only the moment tending to the yaw the ship is balanced by the rudder and the lateral force tending to move the ship toward the near bank is not balanced. The center of gravity of the ship would have a motion toward the near bank and the angle of yaw of the ship would be zero. To overcome the lateral force the ship must be yawed away from the near bank and a greater rudder angle applied. The forces acting on the ship are abnormal, arising from the difference in elevation of the water surface between the ship and the near bank and the ship and the far bank. The data for both Figs. 47 and 48 were obtained in the circulating water channel of the David Taylor Model Basin at Carderock, where the ship model was held stationary and the water flowed past. A three-component balance was used to obtain the balance point of equilibrium, and to measure the lateral force. The tests were conducted in an effort to obtain some critical point in the force pattern and to understand better the various forces acting on the ship. A comparison between data obtained in the towing tank and those obtained in the water channel is given in Fig. 45.

With regard to the discussion by Professor McNown, proof of his treatment of the sinkage problem is given in the official Taylor Model Basin report on these experiments. Certainly this analysis is the proper one for this part of the problem. It was hoped at the beginning of the experiments that a similar solution might be developed for estimating the conditions of force when the

ship is off center, which thus would shorten the test time. Although such a solution does not appear impossible, the experimental technique gave more immediate and visible proof of the ship motions.

J. E. REEVES,[150] M. ASCE, AND E. H. BOURQUARD,[151] ASSOC. M. ASCE.—The scope of the paper was limited to the design of channel and the selection of channel dimensions. Although it is believed that the ship model studies and the investigation and analysis of the existing waterways were entirely adequate for the purposes of selecting adequate channel dimensions, it is realized that the approach is new and that many of the factors involved are deserving of much more extensive investigation and study. Consequently, the limited discussion that this subject attracted was a disappointment.

From the dimensions of tunnel and channel sections proposed by Professor Hobbs, it would appear that he considered the proposed channel overdesigned. The evidence compiled in the studies was such as to prove beyond any doubt that the sections as proposed by Professor Hobbs would be woefully inadequate, both from a navigation standpoint and a security standpoint.

Using the method presented in the Symposium, a determination of channel dimensions adequate for single-direction traffic was made. With an existing ore ship (78-ft beam) as the design ship, the required bottom width is 295 ft at 60-ft depth with side slopes of 1 on 1. Although the ore ship was also used as a design ship in the report for determining the channel width, based on the passing of a second ship, it is not the largest ship that must be able to transit a new canal safely. The 600-ft channel, recommended in the Governor's report, is more than adequate for single-direction traffic of the largest existing naval or commercial vessel, and probably for the largest conceivable ship (vessel requiring lock width of 200 ft). A single-direction channel would have to have dimensions adequate for the safe transit of this large vessel. Based on the largest existing naval vessel, making due allowances for the increased controllability of the navy ship, the required bottom width is 350 ft—just twice the width proposed by Professor Hobbs.

This proposal of two independent channels has other definite disadvantages. It would not be as safe a waterway as the channel proposed in the Symposium, since in that channel each vessel has full use of the entire 600-ft width throughout practically its entire transit. Passings of other vessels would represent a relatively short interval of the transiting time. Only 4% of Panama Canal accidents have been collisions in the relatively narrow channel at Gaillard Cut (minimum width 300 ft); on the other hand, many of the bank-striking accidents could be attributed to loss of control of one vessel following the passing of a second ship. In a channel 600 ft wide, this danger would be greatly reduced because of the increased width of canal available for recovery following the passing maneuver.

The possibility of closure of one or both channels by a ship grounding across the channel would be much greater in narrow channels. Records show

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that 73.5% of the ships transiting the Panama Canal for the period from 1921 to 1940 were longer than 300 ft and that only 0.33% were shorter than 600 ft. The narrow channels, particularly in the tunnel, would be a serious handicap to salvage operations.

The economy of two channels, as compared to the single channel recommended in the report, is very questionable. At relatively shallow depths of cut, the quantities in two channels would exceed the quantity in the single channel depending on the bottom widths and side slopes required. The channel section in the tunnel as indicated in Fig. 106 has an area of approximately 9,600 sq ft. The larger existing naval vessels have submerged areas of as much as 4,100 sq ft. The ratio of channel section to ship area for these larger vessels would be approximately 2.3 to 1. If the vessel were to transit at a speed above a crawl, the "squat" of the ship would be extremely large, and the power to tow and hold the vessel in position would be tremendous. The tunnel section considered in the report investigations had a cross section of 18,000 sq ft, which is believed to be a minimum requirement. The proposal to have passing stations in the tunnel offers tantalizing problems in the handling of ships during the maneuver, particularly when both ships would have to be under the control of towing locomotives from each bank. The tunnel would be an ideal "target" for sabotage. The damage that would be created in the tunnel by the explosion of a munitions ship carrying 10,000 tons or so highly explosive material or the equivalent in atomic explosives can well be imagined. The salvage problems in this confined space would be magnified manyfold over those for a similar explosion in an open channel. An atomic explosion in the tunnel might well make the tunnel useless indefinitely.

Mr. Cole can be assured that careful and thorough consideration was given to all conceivable methods of improving the existing lock canal even though it was obvious, from the security studies, that such methods would not meet the requirements of the law authorizing the report. Space limitations, of course, prohibited the inclusion of any discussion of these studies in the Symposium. From the frequent reference to the paper[119] by Miles P. DuVal, it is concluded that several of the discussers, including Mr. Cole, consider the Pacific Terminal Lake Plan the ideal solution to the canal problem. The advantages of this plan as compared to those of the existing canal are presented in that paper. Readers can rest assured that all merits of the Terminal Lake Plan, with its numerous modifications, were thoroughly considered prior to completion of the report. In fact, probably more so than was indicated by the wording of Public Law No. 280. The lock canal developed by the investigations as most nearly meeting the requirements of the act incorporated the Terminal Lake Plan. It was ruled out as not meeting the requirements of security. Several of the discussers state or imply that the admittedly large expenditures of providing a canal, either sea level or lock type, to assure dependability of operating during times of national emergency, are not justified. This subject is believed adequately covered in the Symposium papers and in the discussions. However, it is considered that a few comments on the eco-

[119] "The Marine Operating Problems, Panama Canal, and the Solution," by Miles P. DuVal, *Proceedings*, ASCE, February, 1947, p. 161.

nomics of modifying the present canal to the Terminal Lake Plan, without benefit of security, are in order.

The advantages of the Terminal Lake Plan can be condensed to the following:

(*a*) Increased capacity;
(*b*) Increased operational efficiency (reduced transit time);
(*c*) Increased ease of operation;
(*d*) Increased safety of transit; and
(*e*) Decreased operational cost.

It is difficult to establish a true value for the first three of these advantages. However, by certain modifications of the existing canal, equivalent increases in capacity and operational efficiency can be obtained. These modifications are estimated to cost $130,000,000, approximately $32,000,000 of which is chargeable to Pedro Miguel Locks and could be saved and used toward the modification of Miraflores Locks to provide a Terminal Lake Plan.

It is impossible to place a monetary value on the increased ease of operation. This advantage would be reflected only in reduced strain on pilots and simplification of dispatching problems. The modifications of the existing locks, as mentioned, would also increase the ease of operation.

Cost of past accidents is a direct measure of the relative safety of various canals. The cost of lock accidents in the existing canal has averaged approximately $1.86 per transit. The maximum capacity of the modified existing canal, or the minimum conversion to a Terminal Lake Plan, would be about 15,700 transits per yr at $1.86 per transit, or an annual cost less than $30,000 per yr. Detailed accident records show that 33% of lock accidents occur at Pedro Miguel. The value of the increased safety of transit that might be credited to the Terminal Lake Plan would be less than $250,000 (33% of $30,000 per yr capitalized at 4%), which is insignificant when considering the cost of modification of the present canal to a Terminal Lake Plan.

From a purely economic consideration, the reduction in cost of operation and maintenance expense is the best argument in favor of the Terminal Lake Plan. The capitalized value of this saving, when combined with the aforementioned savings, is a relatively small part of the cost of providing the minimum Terminal Lake Plan, even without provisions for passing larger vessels. In other words, the construction of a Pacific Terminal Lake Plan is not economically justified at this time, or in the immediate future.

A word of caution is in order for possible users of the channel design methods presented in this paper. In working with the limited data available, many attempts were made to extrapolate and interpolate both model and prototype test data. The authors found that serious inconsistencies resulted in many cases, particularly when changes in ship sizes were involved.

The designer should be familiar with the ships likely to utilize the channel that he is designing. After having traveled on many ships in the Panama Canal, the writers are almost ready to accept the pilots' belief that each ship is an individual and acts accordingly. Frequent discussions of new ideas with licensed pilots will help keep a designer from "going off on the deep end."

As many pilots as possible should be consulted on an idea as they, themselves, are not always in agreement.

The writers consider the design methods presented in the paper as steps in the right direction and not as final design procedures to be accepted without question. Further study and testing will undoubtedly develop desirable modifications in the methods.

WILSON V. BINGER,[152] ASSOC. M. ASCE, AND THOMAS F. THOMPSON,[153] AFFILIATE, ASCE.—The recapitulation, by General Kramer, of the conservative principles used in the treatment of the Cucaracha slope problem is a welcome addition to the Symposium. The confidence in a successful job, expressed by General Kramer, should reassure many who may have noted the doubts which some have tried to cast on the conclusions of this paper.

Mr. Hill is also satisfied that the new and deeper excavation required by a sea-level canal at Panama can be undertaken with assurance despite the great difficulties encountered in the original construction. He properly emphasizes the advances made in the study of the causes and prevention of slides since the tremendous Culebra and Cucaracha failures. These great slides were, indeed, among the most important factors giving impetus to the development of soil mechanics in the United States, and demonstrated the need of a thorough understanding of geologic conditions at any location considered for a large engineering project.

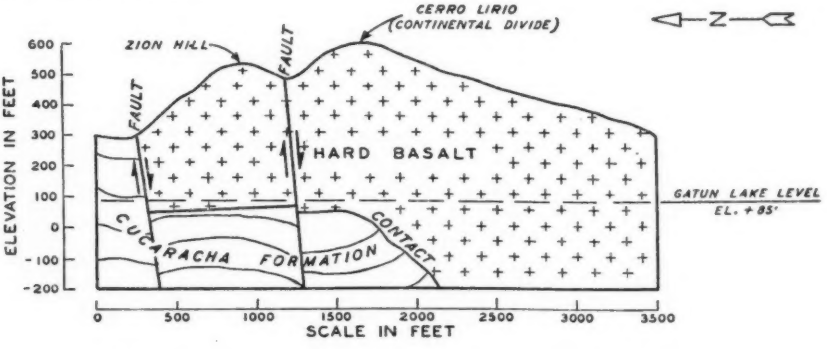

FIG. 119.—IDEALIZED SECTION THROUGH CERRO LIRIO AND ZION HILL

Mr. Justin correctly describes and illustrates the structural relationships of certain of the hills of the Canal Zone in which strong igneous rocks occur in "mushroom-like" structures (laccoliths) with heavy overhanging masses resting on weak sedimentary formations. This feature locally has played an important rôle in the development of Panama Canal slides. However, Zion Hill and the northern half of Lirio Hill at the Continental Divide are of slightly different geologic constitution. Here the igneous rock occurs as a relatively flat offshoot from a deep-seated igneous dike to the south that was later cut

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by a number of minor faults, thereby dividing it into huge blocks that rest on Cucaracha shale (Fig. 119). This condition is even more critical than that described by Mr. Justin in that there is no laccolith neck ("mushroom stem") to act as a pedestal and partly support the weight of the basalt cap. Cross faulting here has eliminated the possibility of a transference of some of the load to the deep-seated igneous base by cantilever action.

Mr. Fucik believes it

> "* * * entirely possible that * * * [slopes for the Cucaracha formation] set by the use of the curve shown in Fig. 64, are too conservative, and that a material steepening of the slopes could be effected without any great risk of incurring catastrophic failures of the banks."

He suggests a curve (Fig. 110) based on the same strength values for the Cucaracha, but using a factor of safety of 1.0 instead of one of 1.3. He is not critical of the use of Fig. 64 to arrive at estimating quantities for the excavation; but he does feel that, if the sea-level canal becomes an actuality, careful and thorough study should be given to the possibility of using steeper slopes in the Cucaracha formation than were presented in the writers' paper.

The possibility of saving 65,000,000 cu yd of excavation is an attractive one, of course. The taking of some calculated risk might thus appear to be desirable; but the degree of risk that is justified in any part of a project is not a matter for final decision by engineering specialists. In a problem such as this one, geologists and soils engineers must determine to the best of their ability the distribution and the structural relationships of the rock formations and the strength characteristics of the various materials, so that curves such as those in Fig. 110 can be developed. On the basis of these investigations and with an awareness of the unknowns to be covered by the factor of safety, those responsible for the project must decide, as a matter of important policy, the appropriate factor of safety. This procedure was actually followed during the Isthmian Canal Studies (1947).

After Congressional authorization of the sea-level canal project, a number of years would be available for further study on the important problem of excavation slopes in the Cucaracha formation. This time should be used for intensive study and research to check the curve of Fig. 64 or to develop a new curve. This work should be done, not only to determine whether the slopes set by the writers are too conservative, but also to be as sure as possible that these slopes were sufficiently conservative. The writers believe that the greatest hope for reducing the excavation quantities lies in the detailed geologic exploration of the critical areas, so that slopes can be modified to take advantage of stronger phases of some formations.

Mr. Sheldon reviews briefly but interestingly the history of the difficulties experienced in building railroad embankments on Atlantic muck. He describes the rugged nature of the rock surface buried under muck at one location with which he is familiar. He then advances the theory that previous failures of embankments on muck have been caused by the material sliding on the steep rock slopes, rather than by failure in the muck.

Detailed studies made of one of the railroad embankments (that just south of Gatun crossing the area known as Stilsons Pond) tend to disprove Mr.

Sheldon's theory. Although a sloping (1 on 9) rock surface underlies the muck on which the fill was built, the history of the construction of the embankment reveals that one of the several slides of this fill was in the uphill direction and that a berm or counterweighting fill had to be built on the uphill side.[154] Furthermore, nothing in the writers' experience confirms the conclusion that the shear strength of the muck-rock contact is lower than the shear strength of the muck itself. The evidence is to the contrary. If the depth of the potential shear surface in an embankment foundation is limited by a rock surface, the stability of the embankment is normally greater than would be the case if the shear surface were free to go as deep as it wished, assuming a constant shear strength for the foundation material. When it is so limited, the shearing still takes place in the muck itself, although possibly very close to the contact.

In any case it may be reassuring to point out that the conditions in the Quebrancha-Monte Lirio-Gatun River area where the Monte Lirio Dam would be built are unlike those at Mount Hope where the Marine Railway was built. At Mount Hope the buried rock surface beneath the muck is steep and highly cut up by canyons running toward the near-by channel excavation, whereas the buried topography at the Monte Lirio site is gently rolling. Lines of borings across the Monte Lirio axis and at the Gatun Dam axis have established the bottom gradient of the old buried channel to be between 5 ft and 6 ft per mile. For all practical purposes, this is flat.

Mr. Cedergren refers to the inconsistencies present in formations in the hilly regions of the Pacific Northwest and mentions intrusions of weak materials. Such conditions do prevail in the Canal Zone along the selected route for a sea-level canal. In fact, although slope curves must necessarily be prepared for each formation to be encountered along the route, it will be necessary to treat each hill as a separate slope design problem.

The writers are grateful to Dean Marston for his kind mention of their paper. His description of the Panama Canal as a "* * * great research laboratory to solve the important problems of safe slopes for a sea-level Panama Canal" is a reminder that in no other branch of civil engineering can engineers learn more from failures than in the field of soil mechanics.

There are those who would ignore any advances made in soil mechanics and engineering geology since 1914. Mr. Kirkpatrick states that deepening the canal cuts by 100 ft is "daringly unprecedented even to the point of recklessness." Had such counsel prevailed in 1905–1906, the present canal would never have been built, for it was certainly a far more unprecedented project than any deepening and widening can now be. It will be a dark day for all human endeavor when men cease to attempt the "unprecedented."

In answer to Mr. Kirkpatrick's unfounded assertion that "Nothing is known, except geological conjectures" of the probable bank and bottom stabilities of the 20 miles of proposed canal now hidden by Gatun Lake, the writers wish to state that investigations were made during the study in sufficient detail for a satisfactory preliminary evaluation of the problems that would be involved in construction. The investigation of the Gatun Lake area included ninety-five holes drilled from the lake surface to explore the muck and to re-

[154] "Annual Report of the Isthmian Canal Commission for Fiscal Year Ended June 30, 1909," Rept. of the Eng. Dept. of the Panama Railroad, Appendix E, U. S. Govt. Printing Office, Washington, D. C., 1909.

cover samples for testing. It was the opinion of the Board of Consulting Engineers that the designs proposed for the slopes through this area were on the conservative side.

In commenting on the proposed dams in the Gatun Lake area, Mr. Kirkpatrick seems unduly alarmed. Gatun Dam was built on Atlantic muck, extending at the deepest point to El. −204.9. The deepest muck located elsewhere to date is at Monte Lirio (El. −163.6).

The possibility raised by Professor Casagrande that "* * * long-time effects may reduce the shear strength of the Cucaracha as a result of very slow swelling caused by reduction in load" cannot be lightly dismissed. The writers would not wish to close the door to the results of future investigations, but they do wish to record something in defense of the curve for safe excavation slopes in the Cucaracha. The evidence that the Cucaracha may lose considerable shear strength with time is too important to overlook, but it is not yet conclusive. This evidence points to a hypothesis such as that developed by Professor Casagrande, and such a hypothesis must be proved or disproved as far as possible before the slopes are actually cut. The writers are in full agreement, therefore, with the recommendations for long-range field and laboratory studies, telltale devices to detect movements in Cucaracha slopes, and the like. They are not convinced, however, that the drop in effective shear strength shown by Fig. 118 is necessarily caused principally by a long-time swelling or weakening effect.

The explanation used and accepted by the writers up to this time has been that successive sliding failures reduce the effective shear strength of the Cucaracha. In opposing this theory, Professor Casagrande states:

> "* * * However, the writer believes that in 1912 at least one half of the mass bounded by the underlying Culebra and on the east and the west by faults, as shown in Fig. 118, was not yet disturbed by the slides and that intermittently additional parts of the mass became involved in the movement. If the assumption is made that the strength characteristics of the undisturbed material have not considerably changed with time and that they are equal only to those governing the slopes in January, 1912, then it can be shown that the mass should have come to rest with much steeper slopes than those existing today."

Although it is true that in 1912 about one half of the material had not yet been disturbed by slides, between that year and 1915 almost all the remaining material did become involved in slides, but the effective shear strengths were reduced, on the east slope, only to 79%, and on the west slope, only to 62%. Since 1915, both slides have been essentially in continuous, although at times almost imperceptible, movement, and during this period the effective shear strength dropped to the very low values cited by Professor Casagrande.

It may well be, of course, that the great reduction of shear strength experienced between 1912 and the present is due to both: (1) The disturbance of slides resulting from oversteep slopes; and (2) the long-time weakening effect deduced by Professor Casagrande.

(Readers who may wish to study the Culebra slide further will be interested in a separate paper presented during 1948 in which the cross section of Fig. 118 and two other sections have been analyzed.[155])

[155] "Analytical Studies of Panama Canal Slides," by Wilson V. Binger, *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, Vol. II, 1948, pp. 54–60.

Professor Casagrande suggests the possibility that the sequence of various layers of the Cucaracha formation in the model slope (on which such reliance has necessarily been placed in the development of new criteria for the design of excavation slopes) is not identical to that in the slopes which failed in the Culebra slides. Exact similarity cannot be proved by available information; however, old geologic sections and recent borings show that both the model slope and the horizon that failed in the East and West Culebra slides are of similar stratigraphic position as indicated by the position of the ash flow marker bed. Both appear to have been equally weakened by minor faults. Since the two locations are close by, it is reasonable to assume that they were originally about equal in strength.

Little information is available on the 100-ft cut that failed in 1907. The late Col. George W. Goethals, M. ASCE, described this slide as follows:[156]

> "In October 1907, a crack developed 50 feet or more from the face of the bank on the west side of the Canal near the old railroad station at Culebra. Subsequent to the cracking, the ground to the west of it showed perceptible settlement, though the elevation of the portion between the crack and the prism remained unchanged; this settlement was accompanied by a bulging up of the bottom of the Canal sufficient to raise the shovel. At the time this occurred the shovel was at elevation 175, and the subsiding portion of the upper bank was at elevation 275. Ultimately the portion between the crack and the prism settled down, sliding into the prism. Subsequently new cracks developed farther back, followed by similar settlements and movements into the cut, involving large masses of material; the same condition developed on the east side."

The writers agree definitely with Professor Casagrande's conclusion that:

> "* * * the strength characteristics of the Cucaracha clay-shale are not similar to those that can be expressed by the Coulomb equation with constant c-values and ϕ-values."

In this case, however, the only alternative to using a c-value and a ϕ-value would have been to use a constant shear strength (that is, a constant value of c and a ϕ equal to zero) determined from the model slope or from the Culebra slides. Such a course would have meant the adoption of impossibly flat slopes for the higher cuts. On the other hand, a constant shear strength would not be a better description of the strength characteristics of the Cucaracha than would the c-value and ϕ-value.

Professor Casagrande's critical review of the methods employed in arriving at the Cucaracha slope design criteria is a valuable contribution to this paper. Although he recognizes that the proposed design curve is the "* * * best that could be accomplished with the tools and the knowledge [now] available," he makes recommendations for further investigations at the time of project authorization which the writers fully endorse.

Acknowledgments.—The soils work of the Third Locks program was first under the direction of Evan W. Vaughan, M. ASCE, and later under that of E. Montford Fucik, Assoc. M. ASCE. Principal assistants in the soil mechanics section at that time were C. K. Smith, Assoc. M. ASCE; A. C. Flach, Jr.; Francis J. Woodsmall, Assoc. M. ASCE; and L. D. Collins. Geologic in-

[156] "The Panama Canal," by George W. Goethals, McGraw-Hill Book Co., Inc., New York, N. Y., 1916, pp. 372–373.

vestigations were under the direction of H. G. Martin from June, 1938, until November, 1941, when he was succeeded by Mr. Thompson. D. F. MacDonald served as consulting geologist for the Third Locks investigations from June, 1938, until his death in May, 1942.

During the Isthmian Canal Studies (1947), important soil mechanics studies were conducted by E. J. Zegarra, Assoc. M. ASCE; H. G. Smith, Jr.; David C. Hall, Jun. ASCE; and R. L. Behrend, Jun. ASCE. C. F. Vincent was in charge of the Soil and Rock Testing Laboratory, assisted by R. L. McLees and George E. Lewis; and W. H. Bussey supervised the sampling of soil and rock. L. H. Henderson, T. G. Moran, J. R. Schultz, J. R. Townsend, S. M. Jones, and others contributed to the geologic studies.

A. CASAGRANDE,[157] M. ASCE, AND W. L. SHANNON,[158] ASSOC. M. ASCE.—Since the publication of the writers' paper, the investigations sponsored by the Panama Canal on the dynamic stress-deformation and strength characteristics of soils and soft rocks were completed and a detailed report was published.[159]

The discussions on the Symposium contained only a few comments relating to the subject of the writers' paper.

Mr. Hill comments on the various other engineering applications where transient loading is important. A more detailed discussion of this question, with particular attention to the stability of slopes subjected to earthquakes, was included by the senior writer in chapter 12 of the final report.[159] Some of the observations made in connection with the traffic tests on airfield pavements, conducted by the Corps of Engineers, are presented in Mr. Cedergren's discussion.

Professor Tschebotarioff points to the results of his tests which show that continuous vibrations cause much greater volume decrease of sand than does static loading. This finding corroborates similar results obtained by other investigators. Perhaps the first to describe this phenomenon was Karl Terzaghi, Hon. M. ASCE, who in one of his early publications termed it the "conservative structure" of sand.

Professor Tschebotarioff criticizes the fact that the transient tests were so conducted that a specimen was first exposed to a hydrostatic stress condition and then the axial load was increased to failure. This method of loading was used for the following two reasons: (1) It is believed that this type of transient test at one end, and the purely static test at the other end, limit the range in which the properties of the material will vary; and (2) tests as proposed by Professor Tschebotarioff complicate an already very difficult type of test. Not until certain fundamental characteristics are fully explored (still far from being accomplished) can one afford to investigate more complicated combinations of load application.

Professor Tschebotarioff cites the case of a bank of varved clay which failed during pile driving operations. As he suggests, it is probably the rock

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[158] Director of Div. Laboratory, U. S. Engr. Office, Portland, Ore.; formerly Research Associate in Soil Mechanics, Graduate School of Eng., Harvard Univ., Cambridge, Mass.

[159] "Research on Stress-Deformation and Strength Characteristics of Soils and Soft Rocks Under Transient Loading," *Soil Mechanics Series No. 31*, Graduate School of Eng., Harvard Univ., Cambridge, Mass., June, 1948.

flour in this material that liquefied as the result of the vibrations from pile driving. The problem of liquefaction of a soil due to collapse of its structure was beyond the scope of the writers' paper; but some information on this point is contained in chapter 12 of the final report,[159] and an instructive example is described by Mr. Cedergren in his discussion.

J. J. ROSE,[160] F. L. DYE,[161] M. ASCE, W. B. WATSON,[162] AND L. T. CROOK,[163] Assoc. M. ASCE.—Mr. McAlpine is correct in his statement that a considerable part of the material below El. 270 could be disposed of economically by truck, belt conveyer, or other land transportation. El. 270 was taken arbitrarily as time did not permit the detailed studies necessary to determine the most economical method for spoiling each yard of the millions of cubic yards. The economical elevation would be dependent on the transverse location of the material, the availability of dry spoil areas, the elevation of ground adjacent to the top of the slope, and the location of the wet spoil areas. Mr. McAlpine objects to dropping material directly into scows from the higher elevations. The writers agree that such a procedure would be both dangerous and infeasible and it was not proposed. The material at the higher elevations would be rehandled to reach the scow loading equipment at scow level.

Perhaps the statement that the average cost of disposal by truck haul would be approximately twice as much as by scow haul should have been qualified by the clause "based on the methods used in computing the cost in the studies." The truck haul costs were based on quotations on first cost of the equipment using equipment ownership schedules advocated by the Associated General Contractors of America to determine the plant rentals. The barge haul costs were based on quotations in which Panama Canal experience was used to determine the plant rentals. It is proposed to allow the contractors to determine the most economical method of hauling by making both land and water disposal areas available under each contract.

Mr. Massey's discussion is extremely interesting. To obtain additional production and cost data concerning the economical handling of rock excavation at the depths required by the bucket-ladder dredge, it is proposed to purchase only one dredge of this type at the beginning of operations and to experiment with the deep dredging rather than to start this phase on a large scale. Only a small percentage of the hard rock is beyond the reach of the dipper dredge and can easily be handled with a clamshell or dragline in the time allotted. The costs of the ladder dredge excavation used in the studies are ample to cover excavation of this hard rock by clamshell in the event its use might be considered advisable.

Mr. Massey states:

"The bucket-ladder dredge purchased by the Panama Canal Authorities about 1912 in Scotland utterly failed to dig blasted rock in the Culebra Cut

[160] Resident Engr., Folsom Dam Project, Corps of Engrs., U. S. Army, Sacramento, Calif.; formerly Chf., Constr. Planning Branch, Special Eng. Div., The Panama Canal, Diablo Heights, Canal Zone.

[161] Chf., Eng. Div., Corps of Engrs., U. S. Army, Charleston, S. C.; formerly Chf., Dredging Section, Special Eng. Div., The Panama Canal, Diablo Heights, Canal Zone.

[162] Engr. in Chg. of Constr., McNary Dam, Corps of Engrs., U. S. Army, Umatilla, Ore., formerly Chf., Dry Excavation Section, Special Eng. Div., The Panama Canal, Diablo Heights, Canal Zone.

[163] Engr., Board of Engrs. for Rivers and Harbors, Dept. of the Army, Washington, D. C.; formerly Chf., Estimates Section, Special Eng. Div., The Panama Canal, Diablo Heights, Canal Zone.

and was replaced as soon as possible by dipper dredges, which were ideal for the work because of the absence of any space or size restriction on the pieces of rock to be handled."

As a matter of fact, the bucket-ladder dredge to which Mr. Massey refers (the *Corozal*) generally performed very creditably. Over a 3-year period from July 1, 1912, to June 30, 1915, material dredged by this machine consisted of 71% rocks and 29% earth, excavated and placed in dumps at an average cost of $0.514 per cu yd. No difficulty was experienced in dredging properly blasted rock. Large dipper dredges were constructed and assigned to work in Culebra (renamed Gaillard) Cut principally to handle (without blasting) large pieces of rock being brought into the cut as slide material. With reference to photographs showing these dipper dredges raising to the water surface pieces of rock too large to pass through the dipper, it should be borne in mind that such oversize rock is the exception rather than the rule. One of the writers has observed an 8-cu-ft bucket-ladder dredge operating under severe conditions in South America bringing boulders to the surface as large as any brought up by the 15-cu-yd dipper dredges at the Panama Canal. Where the dipper dredge could not free such a boulder without dropping or blasting it, the bucket-ladder dredge could raise its ladder clear of the water, reverse its bucket chain, and dump the boulder either on the bank ahead or on a barge for final disposal.

W. G Comber, who was in charge of Panama Canal dredging at the time the *Corozal* was purchased, has stated:

> "In order to avoid the delay and expense of breaking or blasting, it was thought that a high powered modern dredge would cut the softer rocks in the section from Balboa Wharf to Miraflores. On this assumption, the *Corozal* was purchased, and proved amply able to handle this work with an output varying from 3,000 to 10,000 yards of rock per 24-hr day; the harder parts of the soft rocks were handled by cutting 6-inch layers, the softer as much as four feet at one passing of the dredge. The dredge was also very efficient in the rock of the harder shoals after blasting by the '*Toredo*,' but not so efficient in the Culebra Cut work, where large pieces of rock were encountered in places where there was not sufficient depth of water for the dredge to get directly over the rock."

It is true that buckets must be made with a high factor of safety but this practice has been followed for many years. As a result of improved designs, metals, and maintenance methods, present-day dredges very rarely have trouble with bucket-line breakage. In many cases, bucket-ladder dredges obtain 80% or more effective dredging time, year in and year out.

Both the clamshell and dragline types of dredges received full consideration during the course of the Isthmian Canal Studies. Design of a deep-digging dragline dredge, similar to that mentioned by Mr. Massey, was available to those engaged on the Isthmian Canal Studies some time prior to publication of the Symposium.

The advisability of excavating with the special dredges to an elevation satisfactory to pass traffic and to complete the excavation with regular dredges was considered and left for decision during actual construction. There would be no particular advantage in performing the common excavation in this manner.

Mr. Erickson states that the 40-in. hydraulic dredge seems to be too small and that consideration should be given to dredges having, say, 60-in. discharge lines. Actually, consideration was given to dredges having discharge lines up to 60 in. in diameter. However, on the basis of the quantity of common and soft rock excavation set up for hydraulic dredging, it can be estimated that one 40-in. deep-digging dredge would have adequate capacity to meet the 10-year construction schedule. A construction program requiring an increase in common and soft rock quantities in wet excavation would warrant further consideration of dredges larger than 40 in.

In discussing the Panama parallel sea-level canal, Mr. Jarvis refers to "* * * the unavoidable interference with normal canal traffic during the period of construction for the Panama sea-level conversion route * * *." The matter of possible interference was given careful consideration in the studies and the work was so planned that interference with traffic would be negligible. The work can be arranged so that, at the minimum, navigable widths not less than those now existing in Gaillard Cut (300 ft) would be available at all times.

Mr. Edwards questions the need for the somewhat careful cost estimates of canals at other locations and the detail of the sea-level canal estimate. In retrospect, the estimates for alternate canals might be more detailed than required, but, as they were developed concurrently with defense, traffic, geologic, structural, and other studies for all routes, it was impossible to determine in advance just what detail would be required. Forecasting probable future actions of Congress, which would determine when the data assembled would be used, was beyond the scope of these studies.

Mr. Kirkpatrick suggests lowering the channel in Gaillard Cut from El. 40 to El. 37⅓. The lock canal plans studied contemplated channels excavated to El. 37.

The possibilities of using hydraulic giants, breakage of rock by hydraulic methods, and elimination of the necessity of conveying all material to Gatun Lake were discussed by Mr. Dohn. Hydraulic giants are in use on the Panama Canal and have been used for a considerable time, flattening slopes in the areas subject to slides. The material excavated by these machines is washed into the canal for subsequent removal by dredges and it is very small in amount when compared with the project now proposed. The terrain in the vicinity of Gaillard Cut is such that there are no low areas available for disposal of large quantities of spoil by use of hydraulic giants. The only reasonable spoil disposal areas are in Gatun Lake where the water level is at the same elevation as in Gaillard Cut and where spoil disposal requires no lockages.

Breakage of rock by hydraulic methods is not practicable, for one reason, because of the existence of numerous fissures in the rock. Considerable difficulty has been experienced in drilling operations because of the loss of drill water through these fissures. Furthermore, any rock breakage that could be obtained by hydraulic methods would obviously occur along the seams or fissures, leaving large rock fragments that would require secondary blasting at a much higher cost than if the original rock were blasted.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

THE RELATION OF URBAN EXPRESSWAYS TO MASS TRANSPORTATION FACILITIES

Discussion

BY JOSEPH BARNETT

JOSEPH BARNETT,[6] M. ASCE.—Quite properly, this excellent paper emphasizes the need for mass transportation facilities in solving the urban traffic problem but the author's presentation is too strong in implying that advocates of urban expressways contend that they are "* * * the panacea for all these [traffic] ills." Responsible officials recognize that congestion in urban areas will be relieved only by a broad attack on the problem involving several related problems requiring the force and the cooperation of numerous city, state, and federal agencies. The writer has consistently tried to point out and to simplify the broad effort necessary, and has broken it down into the following five-point program:

(1) The improvement of the arterial route system.

(2) The improvement of the streets that must form distributors for all traffic. Nearly all people and goods transported in motor vehicles must be delivered to their ultimate destinations on local streets.

(3) The improvement of the transit system. By no known plan can all traffic be handled by private vehicles. The transit vehicle, whether it be a railroad car, trolley car, or bus, is a much more efficient user of street space in providing mass transportation than is a private car; and, to an increasing degree, the public must look to the transit vehicle to carry a large part of the passenger load, particularly between home and work during the hours of greatest congestion.

(4) The solution of the terminal problem—adequate provision for parking and for the loading and the unloading of goods. It seems unreasonable to expect a city to use its extremely valuable streets for the storage of vehicles for long periods of time, particularly during rush hours, or for the use of trucks loading and unloading goods. Especially in the case of large commercial and

NOTE.—This paper by Curtis G. Bradfield was published in December, 1948, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: April, 1949, by Elmer F. Steigelman, and Harold M. Lewis.

[6] Chf., Urban Road Div., Public Roads Administration, Federal Works Agency, Washington, D. C.

industrial establishments, there seems to be no less justification for requiring off-street loading and unloading areas for trucks than for railroad cars.

(5) The more efficient use of present facilities. In this category the writer would include spot improvements that will make possible more efficient use of streets. Among these might be mentioned widened and improved pavements, markings and signs, more efficient traffic signal control, channelization, and other isolated improvements at points of congestion.

This program will be necessary even though the type of mass transit the author emphasizes (namely, subways) are provided. Subways are economically justified in probably only a few cities. Only five cities in the United States have populations greater than 1,000,000 and not more than fifteen cities have a million people, if the satellite or suburban cities are included. On the other hand, about a thousand cities have populations of more than 10,000. Nearly all these have serious traffic problems. Mass transit for most of these cities consists of gas buses, trolley buses, and trolley cars.

Mass transit is not on the increase as it should be. The impetus given mass transit during World War II has not resulted in permanent gains, and there are signs that mass transit operators are having their old troubles despite the increasing difficulty in moving and parking experienced by drivers of private automobiles in congested cities. Rail operation on its exclusive right of way is excepted, of course. The cause of such difficulties appears to be the vicious cycle in which mass transit operators and the public find themselves. Mass transit operation on city streets is slow and costly because of frequent stops and congestion. As the time of travel on mass transit increases and the fare is raised to meet costs, fewer people ride and more drive, further aggravating congestion.

The arterial routes in most cities are lined with commercial establishments. The routes serve several types of traffic that interfere with one another. Arterial route traffic wants to move fast, without stopping. Neighborhood shopping traffic wants to move in the same manner but only as far as the commercial establishments, where it stops. Buses want to move as rapidly as possible, but they want to stop frequently to pick up passengers. Cross traffic wants to cross without stopping at intersections, and pedestrains want to cross everywhere. Congestion increases as one approaches the center of the city because traffic builds up to higher and higher volumes, width of pavement decreases because of increasing cost of right of way, and intersections become more frequent with higher volume of cross traffic. Street capacity drops sharply just where volume increases and congestion begins. Mass transit vehicles must slow down along with all other vehicles.

The solution being proposed is the construction of separate, free-flowing facilities for arterial route traffic which, in addition to serving such traffic, will release the streets for local traffic including mass transit vehicles. In addition, mass transit of an express nature can then be provided on the free-flowing arterials themselves. The reduced time and cost of mass transit operation on both the new and the old arterials result in a superior service which should encourage their greater use, and the shift back from private vehicles to mass

transit would contribute further to lessening congestion. Here are the ingredients for a reversal of the present cycle with benefit for all.

Railed mass transit is being considered on a few expressways and is being provided on one, the West Expressway in Chicago, Ill. The only logical location for it is in a median of sufficient width to enable stations and platforms to be constructed clear of the high-speed through traffic lanes. Climbing on the part of passengers is unavoidable in such cases but there is increasing evidence that, where volume of passengers justifies it, even this effort will be made unnecessary by the use of escalators, which have a long record of reliable service.

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DISCUSSIONS

EFFECT OF DRIVING PILES INTO SOFT CLAY

Discussion

BY LEONARDO ZEEVAERT, JACOB FELD, JOSEPH C. FEGAN, AND GREGORY P. TSCHEBOTARIOFF

LEONARDO ZEEVAERT,[5] ASSOC. M. ASCE.—The experience of the authors in connection with the remolding of clay due to pile driving has been checked in several details by the experience of the writer in observing the subsoil of Mexico City, Mexico. As an example, certain conclusions can be drawn from the construction of one particular foundation.

General Information.—At the south corner of the intersection of Morelos Avenue and Reforma Avenue an 11-story concrete building was erected on wood piles driven in three sections to a depth of 110 ft. The site and the distribution of piles, and the location of reference points are shown in Fig. 10. The effective area covered by the piles is 4,460 sq ft and the number of piles is 187; therefore, the average area covered per pile is 23.8 sq ft and the average distance from center to center of piles is 4.88 ft.

Before excavation or pile driving, four reference points A, B, C, and D, Fig. 10, were established on the surface of the ground. These points were referred to a fixed bench mark on the old monument of Cristobal Colon on Reforma Avenue, at a distance of about 150 ft from the building.

The site was first excavated to a depth of about 6.5 ft, and holes for the piles were pre-excavated to a depth of 14 ft. The piles were then driven through soft volcanic clay until they rested on a hard and well-packed sandy clay stratum at a depth of 110 ft from the surface of the ground. The soil profile and the variation of the water content and unconfined compressive strength with depth (corresponding to Figs. 2, 3, and 8) are given in Fig. 11. Representative compressibility curves of the soft volcanic clay are shown in Fig. 12. The curve C_u represents the compressibility characteristics of one of the "undisturbed" samples taken at the site by a Shelby tube of 5-in. diameter with a sharp cutting edge. Curve C_r represents the compressibility characteristics of the same material after it was completely remolded at its original

NOTE.—This paper by A. E. Cummings, G. O. Kerkhoff, and R. B. Peck was published in December, 1948, *Proceedings*.

[5] Visiting Research Associate, Univ. of Illinois, Urbana, Ill.; Prof. of Soil Mechanics and Foundations, Univ. of Mexico (on leave of absence), and Cons. Engr., Mexico City, Mexico.

water content. The computed overburden pressure at the depth of 45 ft from which the sample was taken is denoted in Fig. 12 by p_o.

These two compressibility curves show the usual pattern of undisturbed and completely remolded samples for Mexico City volcanic clay. The be-

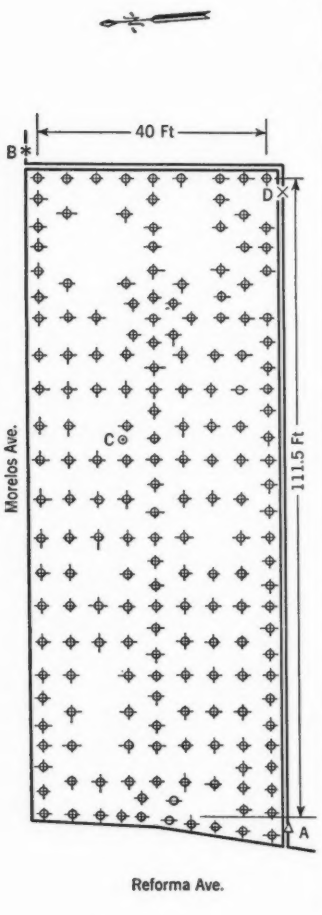

Fig. 10.—Pile Distribution

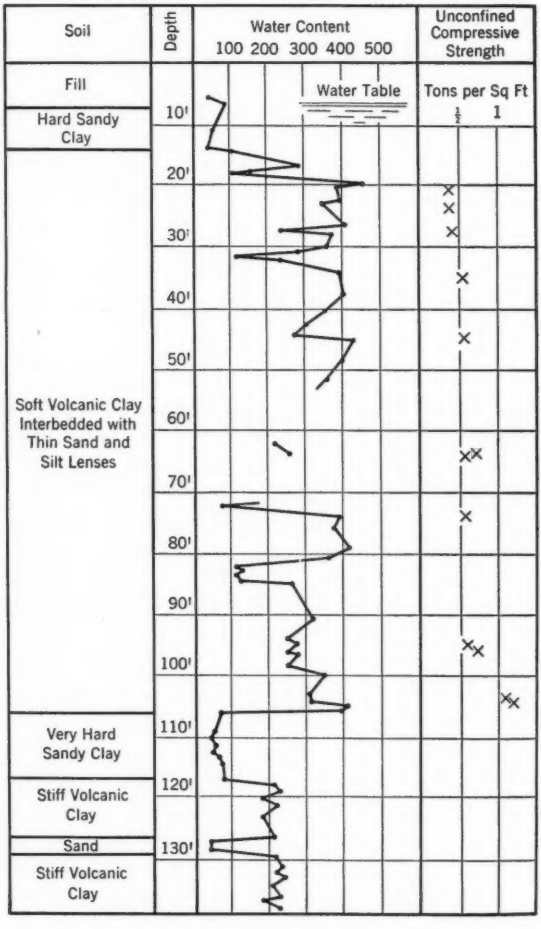

Fig. 11.—Soil Profile

havior of the material in the field is closely expressed by curve C_u. The section AC, Fig. 12, corresponds to the compressibility if the load is too small to produce a marked change in the natural structure of the clay, and the part CB, to the compressibility if the load is great enough to produce a change in the structure. At point C, designated as the "confined compressive strength," a transition occurs. The compressibility increases from values represented by branch A-C of the curve to those represented by branch C-B. For this sample the increase is at least tenfold.

The stress-strain relationship for unconfined compression specimens tested at constant water content is given in Fig. 13 for two samples at different depths. Under these conditions of deformation the material behaves almost elastically up to about 2% strain. Failure follows between strains of 2% and 3%. In the remolded state at the same water content the material bulges under its own weight to a strain of about 5%. The dotted line in Fig. 13 represents the approximate stress-strain relationship for a remolded sample.

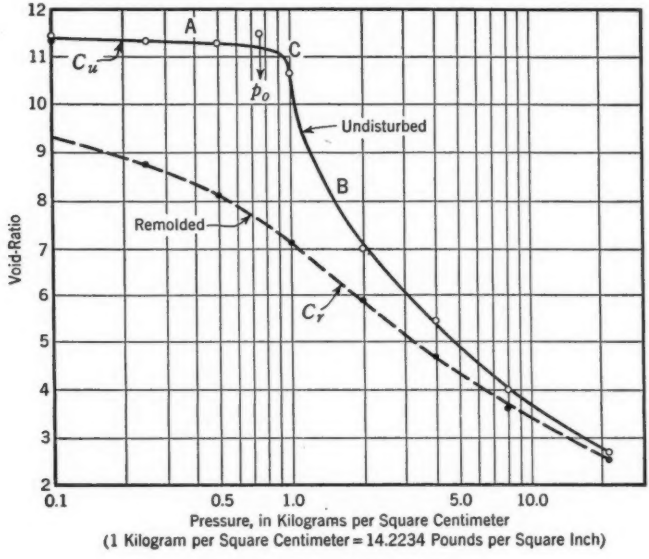

FIG. 12.—COMPRESSIBILITY CURVES

Observations in the Field.—During pile driving the reference points on the surface of the ground moved upward considerably as shown in Fig. 14. Points close to the middle of the lot (curve C, Fig. 14) heaved as much as 13 in., those at the south boundary (curves A and D, Fig. 14), about 10 in., and those at the northeast corner (curve B, Fig. 14), about 11 in. The upward movement of the surface of the ground occurred as rapidly as the piles were driven. After pile driving was finished, the surface of the ground settled back quickly almost to its original position. A tendency of the boundary points (A, B, and D) to subside slightly and the interior points (point C, Fig. 10) to remain heaved, with respect to their original position, may be noticed. The settlement had practically ended one month after the piles were driven. Further observations have shown that, during the succeeding 2 years, the surface of the ground did not settle any further with reference to the bench mark. The important fact that the soil heaved rapidly during pile driving and came back in a very short time to its original position, in spite of the presence of the piles, suggests the following, in support of the conclusions advanced in the paper:

1. The mass of clay did not adhere firmly to the piles by skin friction. No more than a very small dragging force could have developed as a result of the

shearing resistance of the remolded soil close to the pile during the relative displacement between piles and soil.

2. After the soil came back to its original position, no appreciable settlement of the surface of the ground was observed with relation to the fixed bench mark. It appears therefore that the clay between the piles was not substantially altered during pile driving.

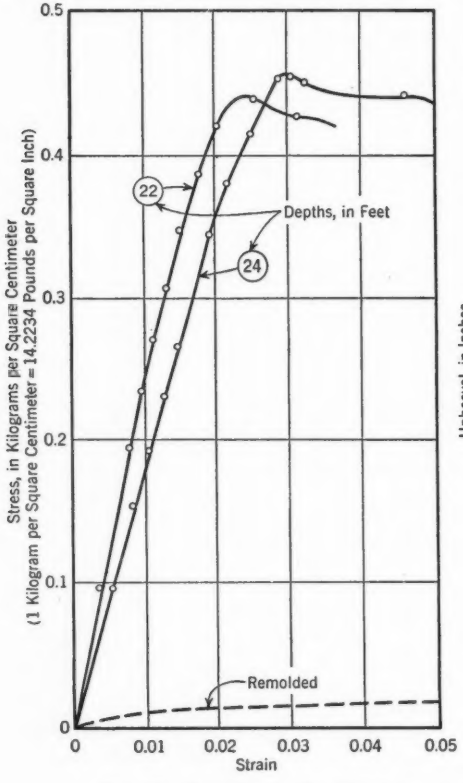

FIG. 13.—UNCONFINED COMPRESSION

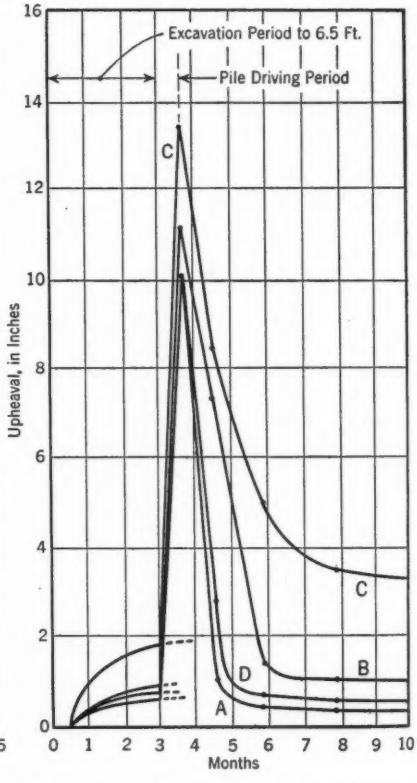

FIG. 14.—OBSERVATIONS DURING PILE DRIVING

Deductions from the Observations.—To test the hypothesis that piles driven in the highly sensitive clay of Mexico City radically disturb that clay, it is first necessary to make a rough estimate of what should happen to the ground surface for different degrees of disturbance in the clay mass due to pile driving. For such an estimate it is necessary to know approximately the frictional force that develops along the piles as the heaved soil subsides to its original position. If the frictional force is larger than, or equal to, the weight of the clay surrounding the piles, the surface of the ground will remain at its heaved position regardless of the degree of disturbance of the clay. Depending on the value of this force, the surface of the ground will come back to a certain position which is a function of the degree of disturbance in the mass of clay and of the dragging force along the piles.

Dragging Force.—The dragging force exerted on the piles during the movement of the soil is small. This is substantiated by the fact that, in Mexico City, structures on point-bearing piles appear to raise above the ground surface, and the sidewalks appear to settle away from the buildings because of the self-consolidation that occurs in the volcanic clay deposit. The sidewalks settle to the very edges of buildings resting on piles, even if the piles are as close together as three to four diameters. If there were appreciable frictional drag, the settlement should decrease near the edges of the buildings.

When a pile is driven to the hard stratum, only a small effort is necessary to push it through the volcanic clay. Usually no vibrations are introduced in the soil because the weight of the pile and hammer, aided by very gentle blows of the hammer, is sufficient to force the pile down to the required elevation where the driving is difficult for a short distance. When the point of the pile is pushed into the clay, the clay is displaced from beneath the point possibly as

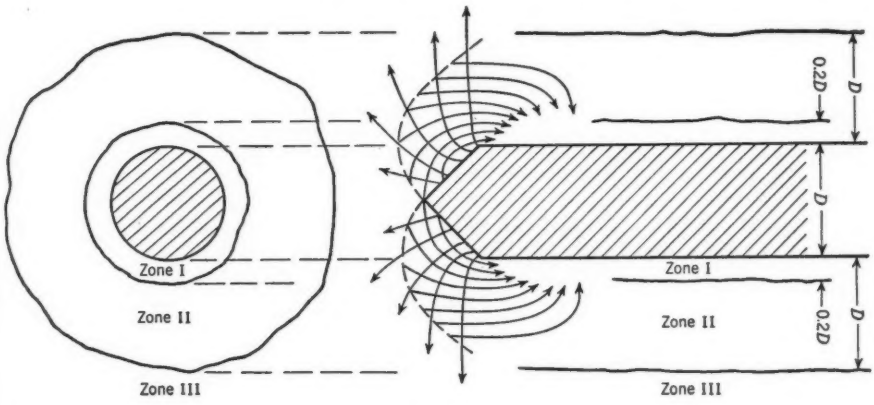

FIG. 15

indicated in Fig. 15. The high pressures imposed by the pile point transform the clay into a highly viscous material that flows upward and away from the point of the pile until it surrounds the shaft in the form of an annular ring. On some occasions, it has been observed that fluid clay has squeezed out to the surface of the ground at the shaft of the pile. Since the process occurs rapidly, the clay changes position at an unaltered water content. Analyzing a horizontal slice above the point of the pile one finds that the new state of the clay surrounding the pile may be classified approximately in three different zones as follows (see Fig. 15):

In Zone 1 the material is probably in a perfectly remolded condition and keeps in movement during pile driving until the pile reaches its ultimate position. If no remolded clay is squeezed up onto the ground surface, the volume of material in the annular ring corresponds exactly to that displaced by the pile. The thickness of the ring is therefore 0.2 *D*.

In Zone II the material is probably considerably disturbed. In this region it is assumed that the clay moves as the point of the pile displaces the soil;

afterward the material should remain almost motionless. The outside diameter of the annular ring corresponding to this zone probably extends to 3 D.

In Zone III the clay is assumed to remain unaltered during pile driving except for a temporary upward elastic deformation due to the pressures caused by the displaced volume of remolded clay during pile driving. Zone III constitutes the remainder of the mass of clay located within the piles. If the linear upward deformation should exceed about 2%, this zone would become somewhat altered (see Fig. 12 and Fig. 13). However, at the site observed, the level readings indicated a maximum deformation of only 1.2%. Therefore, the clay in Zone III retains its elastic, undisturbed character.

Because of the remolding effect the compressibility of the clay surrounding the piles to a distance of approximately D increases as much as ten times. The large hydrostatic excess pressures in the altered clay, caused by pile driving, make this portion consolidate rapidly. This produces a stress relaxation in the rigid, elastically distorted, undisturbed clay and allows it to regain its original position.

The circular boundary of the undisturbed clay moved inward toward the pile because of the consolidation. The movement cannot exceed about 0.12 D, a distance corresponding to the volume of the pile, because the ground surface settled approximately to its original position. Hence, the volumetric strain produced in the remolded clay is not more than 0.12. On the assumption that the initial void ratio of the remolded clay along the pile was equal to the average void ratio in the undisturbed clay before pile driving, and that it was equal to about 9.0, the change in water content in the annular ring of thickness D was approximately $120/2.4 = 50\%$, in which 2.4 is the specific gravity of the solids. Since the initial average water content was 375%, the reduction is of the order of $100 \times 50/375 = 13.3\%$. The usual sensitivity of this clay is greater than about 15. Therefore, a 13% reduction in the water content of the remolded clay could not appreciably increase its shearing strength.

According to the preceding reasoning, one should use the shearing strength of the perfectly remolded clay to estimate the magnitude of the dragging force on the piles due to settling of the clay between them. If an average value of 0.3 ton per sq ft is used (Fig. 11) for the shearing strength of the undisturbed clay, and a minimum value of the sensitivity is taken as 15, then the shearing resistance during the relative movement of the pile and the soil is of the order of $0.3/15 = 0.02$ ton per sq ft. At the site of the observations, the piles are embedded in clay for 90 ft and have an average diameter of 10 in. Hence, the computed total downward force per pile is of the order of $3.14 \times 0.833 \times 90 \times 0.02 = 4.7$ tons. Therefore, the soil would adhere firmly along the piles only if the weight of the surrounding soil were smaller than this quantity.

Test of Working Hypotheses.—Since the dragging force is known approximately and has an order of magnitude of 4.7 tons per pile, it is now possible to test (1) the validity of the thesis of this paper that the soil is not remolded appreciably by pile driving, as well as (2) the concept expressed diagrammatically by Fig. 15. These characteristics can be examined by computing the settlement of the ground surface and comparing the results with the observations. First, assume the clay mass to be completely disturbed during pile

driving, and the mass of clay between the piles to act as a perfectly remolded material. The material will consolidate to a void ratio in accordance with that determined by a completely remolded specimen in the laboratory. Secondly, assume the clay to behave as a remolded material only to a distance from the shaft of the pile to one diameter, and the remainder of the clay between the piles not to change substantially.

The deformation of the surface of the deposit because of a change in volume compressibility and a change in effective stress may be expressed by

$$S = \frac{\Delta e}{1 + e} H + m_v H \Delta p \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

in which S is the surface deformation; H is the thickness of the deposit, equal to 90 ft; p is the average effective stress; and m_v is the average coefficient of unit volume change of the deposit in the unaltered condition, equal to 0.045 sq ft per ton.

Concept (1) will be tested by a computation for the center of the area. The average overburden pressure including the relief of stress due to excavation is 0.672 ton per sq ft. One pile, representing an average area of 23.8 sq ft, offers a frictional resistance of 4.7 tons. Hence, the approximate change in stress in the deposit will be $-4.7/23.8 = -0.197$ ton per sq ft. The initial void ratio from the compressibility curve C_u, Fig. 12, is 11.2 for a pressure of 0.672 ton per sq ft and the final void ratio taken for a pressure of $0.672 - 0.197 = 0.475$ ton per sq ft is found from curve C_r, Fig. 12, to be 8.2. Therefore, $\Delta e = 3.0$; and Eq. 1 gives $S = \frac{3.0}{1 + 11.2} \times 90 - 0.045 \times 90 \times 0.197 = 22 - 0.7 = 21.3$ ft; that is, the ultimate subsidence of the surface of the ground would be roughly 21 ft if settlement of the Mexico City volcanic silty clay occurred contrary to the Cummings-Kerkhoff-Peck concept. Drainage conditions are favorable in the deposit. Therefore, at least a large percentage of this subsidence should appear in the first months after pile driving. To the knowledge of the writer, there has never been reported a subsidence of the ground due to pile driving in Mexico City that amounts even to a small fraction of this value.

The second concept (that illustrated by Fig. 15) leads to the following values, in tons per square foot, for the center of the lot:

Description	Pressure
Overburden pressure considering excavation	0.672
Decrease in pressure due to frictional resistance of piles	−0.197
Increase in pressure in the undisturbed clay due to the area occupied by pile and remolded material (26%)	0.175
Final pressure	0.650

Increment change in pressure is $0.650 - 0.672 = -0.022$ ton per sq ft.

Since the central portion of the clay remains unaltered, $\Delta e = 0$, and Eq. 1 becomes: $S = 0.045 \times 90 \times (-0.022) = -0.089$ ft $= -1.07$ in.

If similar computations are made for the corner of the lot, a value of + 0.25 in. is obtained for the subsidence of the surface of the ground.

Conclusion.—The results obtained in the second computation, although only a rough estimate, show with reasonable accuracy the general trend and order of magnitude of the movements that have been observed in the field, and are in agreement with general experience in Mexico City. The conclusion of the authors that the disturbance is limited to a zone within a few inches from the pile and that the main volume of the clay mass remains practically undisturbed during pile driving appears to be proved. Therefore, the writer believes that the real phenomena in the field are in close agreement with the second concept, illustrated in Fig. 15.

Finally, according to the concept of complete remolding, structures on spread footings or rafts adjacent to sites where pile foundations are driven should begin to settle because of the disturbance. The writer has never observed this phenomenon, although the compressibility of the clay is so great when remolded that any such occurrence would certainly have attracted his attention.

JACOB FELD,[6] M. ASCE.—Just about two hundred years[7] ago John Muller wrote—

> "As the foundations of all buildings in general, are of the utmost importance, in respect to the strength and duration of the work, we shall enter into all the most material particulars, which may happen in different soils, in order to execute works with all the security possible, because many great buildings have been rent to pieces, and some have fallen down, for want of having taken proper care in laying the foundation."

—and it is in this approach to the foundation problem that the authors have conducted their study to determine how a clay soil is altered by the driving of closed end piles.

A summary of the five recommendations and comments made by Muller seem very pertinent to the authors' conclusions:

(1) Use proper augers to bore holes from 10 ft to 15 ft deep in order to discover the nature of the soil and its hardness.

(2) If the soil is gravel or hard stiff clay, no sinking will occur.

(3) If the soil is not uniform, lay a grate of timbers crossed two ways, sometimes boarded over with 3-in. wood planks; sand needs such grates.

(4) If the soil is first soft and hard below, then drive piles and lay a grate above them; drive a few piles to determine the proper length before cutting all the piles.

(5) Sometimes a bed of gravel or clay has soft mud below, and it is dangerous to drive piles, then build on a timber deck and let it sink to bearing (a method used in Russia and Flanders with good results).

The authors have contributed to this centuries old search for the "proper care in laying the foundation." It does not seem that they intend to convey an impression that the clay was not altered, for such an impression would not be substantiated by the results.

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[6] Cons. Engr., New York, N. Y.

[7] "Treatise Containing the Practical Part of Fortification," by John Muller, A. Millar, London, 1755, p. 160.

Under the conditions of the test, the authors' conclusions that the soil was not weakened seem warranted.

The spacing diagram shows that a pile occupies 20 sq ft, of which the pile displaces 5%. Of the 65 ft of clay, between El. 505 and El. 570, the volume replaced by the pile is 65 cu ft. This volume displacement would indicate a rise of 3.40 ft in the earth's surface, if no change occurred within the soil mass. The paper reports that the "ground surface between the piles heaved about 2 ft during the driving operations." The remaining 1.40 ft indicates an average compressive strain of more than 2% in the 65-ft layer of clay. The intensity of the resulting stress is indicated by the squeezing of as much as 1 ft of the clay into the casing of test boring P-1 taken near the pile 1 month after driving. No such phenomenon is reported in the P-11 boring. Is it possible that the stress condition was dissipated in the 10-month interval? This would explain the difference in the distortion of layers shown in the samples in Fig. 6, but it is hardly proof that the clay is not affected by the pile driving, or that the pile may not be affected by this change in internal soil conditions.

Another difficult item to explain is the moisture loss, unless it is assumed that the solid fraction of the soil displaced is squeezed into the immediately adjacent soil and that the soil was not fully saturated, so that the gas content was compressed sufficiently to make room for the additional solid material. This change may not be equivalent to remolding, but it certainly changes the soil characteristics and may have unexpected effects on the piles. This type of soil stress condition has been known to cause heaving of piles.

The foregoing comments are not in criticism of the authors' study. The costs of such field and laboratory tests are quite high. The piles considered are definitely end bearing as far as load—carrying capacity is concerned. Had the piles been driven as friction piles, a record of settlements on the piles or on the structure carried by the piles could have been used to correlate changes in soil with change in vertical position. That is, after all, the subject about which the paper should be chiefly concerned. As Muller pointed out, if the soil is "first soft and hard below," then drive piles and place the load on them. However, when the soft mud is below, it is dangerous to drive piles. It would be worthwhile to have the authors' conclusions as to whether the alteration of soft clay resulting from pile driving can be depended upon as an improvement of the soil bearing value at the depths where the piles transfer load to the soil. Even Muller knew that piles in such soils increased the soil bearing value near the surface; and he recommended a French method consisting of a pile enclosure with the soft soil dug out with a "machine scoop," not deeper than 6 ft and surfaced with a crib if the soft muck ran deeper, and also a surface of cement concrete "which sets under water."

It is difficult to believe that this expensive research program was undertaken only to disprove the statement that soft clay is completely remolded by pile driving. It is hoped that the authors will publish any other practical conclusions observed in this work.

JOSEPH C. FEGAN,[8] JUN. ASCE.—This paper contains the first experimental data that can be used to evaluate the analysis of clay structures pre-

[8] Cons. Engr., New York, N. Y.

sented[4] by A. Casagrande, M. ASCE, in 1932. The fundamental contentions of Mr. Casagrande that—"* * * pile driving in consolidated or partly consolidated clay remolds the clay and thereby increases its compressibility"—are substantiated by the experimental data in the paper. The authors' interpretation of the data, and their conclusions, conflict with the theory developed and the assumptions made by Mr. Casagrande. The principal variance of the two papers is in the estimate of the extent of the zone of remolding. The paper uses unconfined compression test strength as the sole quantitative basis of determining the amount of remolding. However, a measure of the increase in compressibility is the dominant need, since failure in the type of foundation being considered is generally caused by large settlements. Confined consolidation tests most accurately simulate the condition of the soil in the ground and directly measure the consolidation of the sample. By plotting the consolidation, under a given load, of undisturbed samples, and samples at various distances from the pile, the extent of the zone in which detrimental consolidation takes place can be determined. This method is used by Mr. Casagrande, but he was limited by the fact that only the undisturbed sample and the completely remolded laboratory sample were available. If confined consolidation tests could be made on the samples taken by the authors, the missing information between these two extremes would be furnished. Until such tests are made, the results of Messrs. Cummings, Kerkhoff, and Peck cannot be used with confidence to evaluate the accuracy of the conclusions of Mr. Casagrande.

On the basis that dried samples, although severely distorted, retained evidence of their original stratification, the authors conclude that complete remolding did not occur. The significance of remolding in clay is that grain arches, which are the principal supporting elements of the material, are broken down so that the grains are free to move and consequently can consolidate partly. This process does not require the mixing of soil from the component strata, but can occur within each stratum so that the grain arches are broken down with only a small relative movement of adjacent grains and a destruction of the cementing material at grain contacts. In such a situation the strata of the dried sample as distinguished by color, texture, etc., would not be disarranged although reduced in thickness. Thus, the principal supporting capacity of the clay would be lost without the intermingling of component strata.

By eliminating all but a horizontal drainage path away from the pile, the authors observed that the process of consolidation initiated by pile driving would be arrested by the lack of suitable drainage. The possibility of a vertical drainage path at the surface of the pile was not considered, however. In 1927, Karl Terzaghi, Hon. M. ASCE, concluded[9] that, when piles are being driven in clay, displaced pore water is forced upward between the surface of the pile and the impervious clay to a free water surface above. Pore water forced from a stressed clay mass would similarly be forced vertically along the pile surface to the nearest free drainage surface. In this situation, the static

[4] "The Structure of Clay and Its Importance in Foundation Engineering," by A. Casagrande, *Journal*, Boston Soc. of Civ. Engrs., April, 1932.

[9] "The Science of Foundations—Its Present and Future," by Charles Terzaghi, *Transactions*, ASCE, Vol. 93, 1929, p. 270.

load forces on the pile would be equivalent to the dynamic force of pile driving considered by Mr. Terzaghi. From observation of Fig. 4, it can be noted that the clay at the surface of a pile is so remolded that numerous cracks and groves exist. These combine to form an open channel flow path around the pile, that offers less resistance to flow than does the geometrically shorter route to the center of the impervious clay mass. Thus the process of consolidation initiated by pile driving can continue through drainage surfaces provided by the piles themselves.

Conclusions.—Although the paper provides much needed experimental information, there is still need for a confined consolidation compression test on the samples obtained and for a settlement calculation for a typical friction pile foundation at the site of the samples. Interpretations and conclusions loosely considered as observations may mislead the engineering profession into neglecting the valuable analysis of the effect of driving friction piles in clay presented by Mr. Casagrande.[4]

GREGORY P. TSCHEBOTARIOFF,[10] M. ASCE.—Interesting and valuable data on the effect of pile driving on the water content and on the unconfined compressive strength of a clay layer are presented in this paper. However, it is to be noted that the authors did not offer any direct data to support the thesis that the ground surface between the piles did not subside. It is comparatively easy to obtain this information in conclusive form by direct measurements of settlement provided they are undertaken simultaneously with other forms of study at the start of a project. The writer is familiar with a case in which such direct measurements were made somewhat later but nevertheless produced irrefutable evidence in support of conclusions diametrically opposed to the conclusions submitted by the authors on the important point of the ground surface settlements.

This case was as follows: The structural framework of a large 4-story building rested on 120-ft steel pipe piles which had been driven with closed end through laminated soft varved clay of glacial origin to a good point bearing on an underlying hard stratum. The building had a subbasement, which involved excavating from 3 ft to 4 ft of the soil and reducing the load on the ground by a corresponding amount. Therefore, the original designers of the building deemed it safe to let the basement floor and partitions rest in part on the ground itself, rather than to have them entirely supported by girders connected to the footings that capped the piles. Soon after the building was completed and occupied, signs of distress appeared in the basement in the form of structurally unimportant but unsightly cracks in the floor and in the partitions. The nature of these cracks indicated the probability that part of the concrete floor slab which rested directly on the ground between the pile groups was losing its support there. Holes were then drilled in the basement floor slab and reliable bench marks were established in the upper few feet of the clay. Precise measurements were performed which indicated that the surface of the clay was settling in respect to the elevation of the pile capping beams. The settlement was still continuing at a decreasing rate after several years had elapsed. All

[10] Associate Prof. of Civ. Eng., Princeton Univ., Princeton, N. J.

evidence pointed to a gradual process of reconsolidation of the clay under its own weight as a result of its partial remolding by the pile driving. The steel pipe piles were driven in one single welded section. Thus drainage of the clay was possible only upward and downward.

In the case just described the distance between pile groups was four to five times greater than the distance between the piles in the case described by the authors. A lesser degree of remolding should have been expected; nevertheless ground surface settlements were actually measured between the pile groups. Otherwise the conditions were similar. Therefore the contrary conclusions of the authors concerning ground surface subsidence are subject to question unless direct and quantitative evidence is produced to support them. It is too late now to start settlement measurements since equilibrium has probably already been reached in this case. The writer has an hypothesis to offer

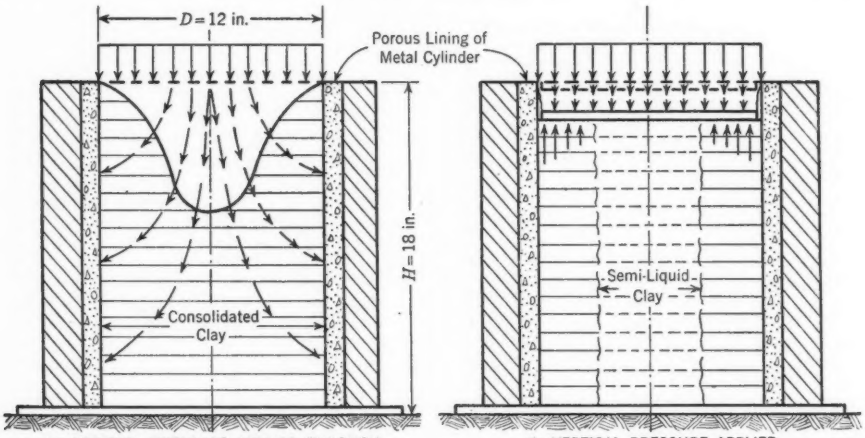
D = 12 in.
Porous Lining of Metal Cylinder
H = 18 in.
Consolidated Clay
Semi-Liquid Clay
(a) VERTICAL PRESSURE APPLIED THROUGH A FLEXIBLE RUBBER MEMBRANE
(b) VERTICAL PRESSURE APPLIED THROUGH A RIGID PLATE

FIG. 16.—THE EFFECT OF BOUNDARY CONDITIONS ON THE SURFACE DEFORMATIONS AND ON THE CHANGE OF WATER CONTENT OF A CLAY CONSOLIDATED IN THE LABORATORY

which may explain, in a manner quite different from that of the authors, the observations concerning water content changes. This hypothesis is based on the results of tests performed in the writer's Soil Mechanics Laboratory at Princeton University in Princeton, N. J., with an apparatus termed "lateral earth pressure meter." [11,12]

The lateral earth pressure measurements themselves, as well as the testing techniques which were developed in order to meet the special conditions created by the deformations of the surface of the consolidating clay, are not pertinent to the present topic and therefore will not be discussed. The observed deformations of the surface of the originally fluid clay, however, are of direct interest and are illustrated by Fig. 16. When the vertical pressure was applied

[11] "Special Features of Large-Scale Earth Pressure Tests," by Edward R. Ward, John R. Bayliss, and Philip P. Brown, in "Lateral Earth Pressures on Flexible Retaining Walls: A Symposium," *Proceedings*, ASCE, January, 1948, pp. 49–57.

[12] "Effects of Boundary Conditions on Lateral Earth Pressures," by Gregory P. Tschebotarioff and John David Welch, *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, 1948, Vol. III, pp. 308–313.

by compressed air to the originally plane surface of the clay through a flexible rubber membrane at all stages of consolidation, as well as after its completion, the surface of the clay was found to show a pronounced dip at the center of the cylindrical apparatus. This dip could be explained only by the restraining effect of shearing stresses developed along the rigid but pervious vertical walls of the device. Thus the rate of the settlements of points at the clay surface was governed not by the rate of consolidation of the clay beneath each point of the surface, but by plastic deformations of the clay mass which must have followed trajectories approximately indicated by the dash-dotted lines and arrows in Fig. 16(*a*).

An entirely different condition was observed when a rigid steel plate was interposed between the clay surface and the rubber membrane. It was found, after completed consolidation, that the steel plate had been supported by a hollow outer cylinder of consolidated clay with a central shaft of semi-liquid clay of the original consistency, as illustrated by Fig. 16(*b*).

The foregoing findings and observations, which were made in the laboratory, will now be extended to illustrate a somewhat similar hypothetical condition in the field, which may arise when concrete piles are more pervious than the surrounding clay and can serve as rigid vertical drains for that sensitive clay remolded by the pile driving and reconsolidating under its own weight. This hypothetical condition is illustrated by Fig. 17. The piles would be surrounded by cylinders of reconsolidated clay of a water content smaller than the original one. These cylinders of denser clay would support the remolded clay between them (as illustrated by Fig. 17) somewhat similarly to the way concrete columns support flat slabs in a building.

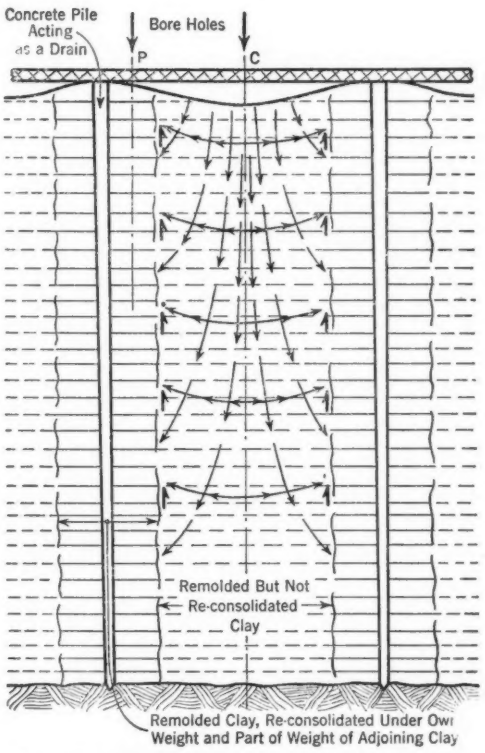
Concrete Pile Acting as a Drain
Bore Holes
P
C
Remolded But Not Re-consolidated Clay
Remolded Clay, Re-consolidated Under Own Weight and Part of Weight of Adjoining Clay

FIG. 17.—SKETCH ILLUSTRATING HYPOTHETICAL CONDITION IN THE FIELD ANALOGOUS TO A COMBINATION OF THE LABORATORY CONDITIONS ILLUSTRATED BY FIGS. 16(*a*) AND 16(*b*)

Many observations reported by the authors agree with this hypothesis should one wish to apply it to the case described in the paper. For instance, the clay in the borings P next to the piles was under such high pressure that it squeezed into the casing. On the other hand, this did not happen in the C-

borings midway between the piles. This reaction should be expected from Fig. 17 since, according to the hypothesis which it illustrates, the remolded and not reconsolidated clay in that region would be partly supported by the cylinders of denser clay around the piles.

A prerequisite for the validity of the hypothesis illustrated by Fig. 17 is the drain-like action of the piles. The authors claim that this was not possible in the case reported by them, but their reasoning is not entirely convincing. The waterproofing of the joints of the corrugated metal shell might have appeared fully effective, it is true, during the few hours (or even minutes) that elapse between the driving of the shell and its filling with concrete; but the

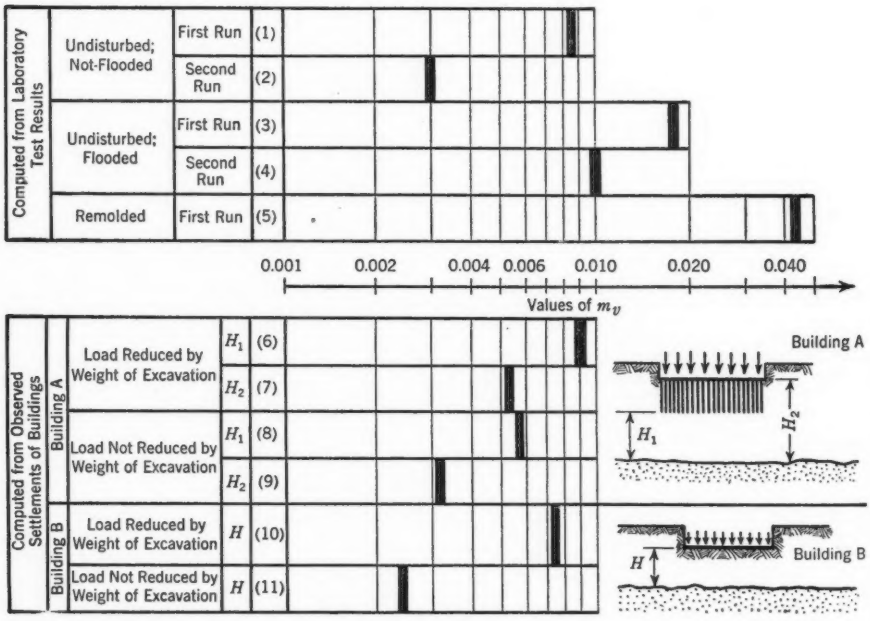

FIG. 18.—COMPARISON OF VALUES OF THE COMPRESSIBILITY COEFFICIENT m_v COMPUTED FROM THE AVERAGE RESULTS OF DIFFERENT LABORATORY CONSOLIDATION TEST PROCEDURES AND FROM SETTLEMENT OBSERVATIONS OF BUILDINGS A AND B

clay itself would help to produce that impression since the visible discharge through it would be very slight within such a short time even in an open and only partly lined pit. These same joints, on the other hand, might permit an appreciable leakage through them over a period of a month. The condition of a thin layer of soil in immediate contact with the skin of the corrugated metal shell of the pile might be such that some seepage in a vertical direction toward the joints could be facilitated there. For instance, it is conceivable that some of the more pervious fill material from the surface filled the hollows of the corrugations of the shell during the pile driving and was carried down with the pile.

For all the aforementioned reasons, the writer cautions against the unreserved acceptance of the thesis of the authors according to which a clay remolded by pile driving will not reconsolidate and settle under its own weight. On the other hand, the writer fully agrees with the contention of the authors that A. Casagrande, M. ASCE, exceeded reasonable limits in his original 1932 forecast of the extremely detrimental effect of pile driving into clays; and especially so in his comparison with other possible sources of disturbance. This opinion is based on the results of a combined field and laboratory study which the writer performed in 1946.[13] Fig. 18 compares the values of the coefficients of volume change m_v as obtained from different procedures of laboratory consolidation tests and from the observed settlements of two buildings. The laboratory m_v-values were obtained from the formula—

$$m_v = \frac{a}{1 + e_1} = \frac{e_1 - e_2}{\Delta p\ (1 + e_1)} \quad \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

in which a is the coefficient of compressibility $= \Delta e/\Delta p$; e_1 is the initial void ratio; e_2 is the final void ratio; and Δp is the increment of unit vertical pressure. The laboratory m_v-values marked "second run" were obtained by the procedure described by the writer elsewhere[14] in connection with studies of swelling clays. The preconsolidation load of such clays was determined first. Then a new specimen of the clay was loaded up to the preconsolidation load, unloaded to the value of the weight of the overburden, and loaded again—this second loading being then referred to in Fig. 18 as the "second run."

Several alternatives had also to be considered when computing the m_v-values from the observed settlements s. In the case of building A, which was supported by frictional piles, the exact average depth H of the compressed layer is not known. For that reason computations were performed for the limit values H_1 and H_2, the actual value of H having an intermediate value. Also it was not known whether the clay had fully readjusted itself to the new stress conditions after the completed excavation of the basement. If it were known, then the full load of the building, without reduction by the weight of the excavation, should be used for the computation of Δp in Eq. 3:

$$m_v = \frac{s}{\Delta p\ H} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

On the other hand, if the clay had neither expanded nor heaved after completion of the excavation, the reduced load should be used for the computation of Δp. Both limit conditions are given in Fig. 18. It should be noted in this connection that building B, Fig. 18, is the same structure for which the time-settlement curve was given by Mr. Casagrande.[15] That diagram indicates zero settlement

[13] "Comparison of the Extent of Disturbance Produced by Driving Piles Into Plastic Clay to the Disturbance Caused by an Unbalanced Excavation," by Gregory P. Tschebotarioff and James R. Schuyler, *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, 1948, Vol. II, pp. 199–205.

[14] "Comparison Between Consolidation, Elastic and Other Soil Properties Established from Laboratory Tests and from Observations of Structures in Egypt," by Gregory P. Tschebotarioff, *Proceedings*, 1st International Conference on Soil Mechanics and Foundation Eng., Cambridge, Mass., 1936, Vol. I, pp. 33–36.

[15] "The Structure of Clay and Its Importance in Foundation Engineering," by A. Casagrande, *Journal*, Boston Soc. of Civ. Engrs., April, 1932, Fig. 28.

in August, 1929, when the weight of the building reached the weight of the excavation. However, a copy of the actual settlement record for August 26, 1929, examined by the writer, indicated measured settlements which varied on that date from 0.25 in. to 1.3 in. in different parts of the building, with an average value of 0.7 in. Thus, some heave of the clay surface must have occurred previously during excavation, and the actual condition in the field would lie between the two limits indicated in Fig. 18.

With this in mind, it can be noted from Fig. 18 that the value of the m_v-coefficient of a completely remolded laboratory sample (in line (5), $m_v = 0.0430$) is approximately five times greater than the largest possible value based on actually measured settlements (in line (6), $m_v = 0.0090$) of that coefficient for building A which was supported by piles. It is approximately seven times greater than the average value of lines (6), (7), (8), and (9) ($m_v = 0.0056$) which the writer believes to correspond more closely to actual conditions. At the same time this latter value is almost identical with the average value (lines (10) and (11), Fig. 18 ($m_v = 0.0050$)) for building B which was not supported by piles.

All laboratory tests on which Fig. 18 is based were performed on samples obtained by Shelby tube samplers 3.5 in. in diameter. A detailed discussion of the significance of the results is beyond the scope of the present paper. It should be mentioned, however, that only the samples for which the photographs showed no distortion at all were used for final evaluations of undisturbed values.[16] Their average strain at unconfined failure was 9.3% for a 10-min to a 20-min duration of test. On the other hand, samples of sensitive clay of the type shown in Fig. 5, although claimed by the authors to be "practically undistorted," in the writer's experience would have to be considered partly remolded with much higher strains at failure, 30% to 40% lower strengths, and greater compressibility. It is to be hoped that the closing discussion will give the strains of specimens at failure. Presumably the use of only 2-in.-diameter tubes is responsible for the condition of the samples illustrated by Fig. 5.

The writer surmised that the almost identical values of the m_v-coefficients of buildings A and B (Fig. 18) might be explained by the remolding effects accompanying the distortions of all underlying layers, which distortions could be induced by a heave at the bottom of the excavation, such as appears to have occurred in the case of building B, whereas the remolding caused by pile driving might be much more localized. This hypothesis was confirmed in a qualitative manner by a three-dimensional model study with gelatine performed in 1947 by James R. Schuyler.[13]

To summarize, the writer feels that, although the authors are right in pointing out that Mr. Casagrande originally exaggerated the detrimental effects of pile driving on clay, the authors themselves have now gone too far in the opposite direction.

[16] "The Determination of the Shearing Strength of Varved Clays and of their Sensitivity to Remolding," by Gregory P. Tschebotarioff and John R. Bayliss, *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, 1948, Vol. I, Fig. 1.

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DISCUSSIONS

PROPERTY SURVEYS MUST FIT THEIR TITLES

Discussion

BY ROBERT F. BORG, AND S. A. BAUER

ROBERT F. BORG,[3] JUN. ASCE.—The stimulating remarks in this paper are welcomed for their profitable exposition of the several features useful to those engaged in property surveys. It is to be regretted that the paper was kept so brief as to permit only generalizations with regard to some of the problems of descriptions which are not true under all circumstances.

The primary purpose of property surveys is to insure that the owner of a piece of land, and the entire world, know the exact location and extent of his property, chiefly as embodied in deeds. When disputes over descriptions arise, if they cannot be settled by qualified land surveyors, they are resolved by courts of law. Since the law is the final arbitrator in these instances, its rules, whether or not based on logic and reason, must temper the thinking of surveyors.

The author states that if the ownership is established (as in many cases) by a description drawn from a plat, or by compilation from record sources, the monuments must be assumed to be confirmatory and subject to replacement. This categorical statement can be both misleading and incorrect. If the description, as embodied in the deed, refers to monuments not in existence at that time, but which will be erected at a later period, such monuments, once erected, will bind the parties should there be any discrepancy between the deed description and the actual monument locations (Lerned versus Morrell, 2 N.H. 197; Emery versus Fowler, 38 Me. 99).

Naturally, where there are no references to any monuments in the property description in the deed, it is assumed that the taker under the deed did not intend to be bound by any incorrectly located monuments that may have existed (Murdock versus Chapman, 9 Gray (Mass) 156; White versus Loring, 93 U.S. 514). The courts have uniformly upheld this latter contention, and the author's statement is correct under those circumstances.

Again, the author states that, regardless of description or record, monuments govern only if the original survey is precedent to the record. This statement must also be considered true only for certain factual situations. If, in their

NOTE.—This paper by William C. Wattles was published in February, 1949, *Proceedings*.

[3] Estimator, Psaty & Fuhrman, Inc., Contractors and Engrs., New York, N. Y.

deed description, the parties do not in any way refer to monuments then in existence (or later to be erected) and if, subsequent to the date of record, they either establish monuments or run a line, holding that line and occupying the land defined by it, for a long time, then the line or monument thus erected will bind the parties where the deed description is ambiguous (Baldwin versus Brown, 16 N.Y. 359; Breaker versus Woolsey, 149 Mich. 86). On the other hand, where the deed description is certain and unambiguous, as the author indicates, any line or monument subsequently established is not binding on the parties (Iverson versus Swan, 169 Mass. 582; Hall versus Eaton, 139 Mass. 217), except by adverse possession (under common law, this would be open and notorious possession for more than 20 years) (Burr versus Smith, 152 Ind. 469; Roberts versus Birks, 223 Ill. 291).

In the last analysis, it is the true intent of the parties that will govern in any particular case. Thus, where, through some misfortune, the survey description made for a deed is at variance with the actual locations of physical features or monuments, the court will weigh all the facts and circumstances in an endeavor to decide the case in a manner that will best express the probable intent of the parties who used the property survey (White versus Loring, 93 U.S. 514, 524).[4]

The author is to be congratulated for bringing to the attention of the profession the need for thoughtful and accurate property surveys.

S. A. BAUER,[5] ASSOC. M. ASCE.—A scholarly exposition of a basic principle of property surveying is presented by Mr. Wattles. The paper is particularly welcome and important to the practicing surveying profession in that it stresses the true essence of property surveying—title and law, research and logic—and relegates to a proper position of secondary importance the mechanics of the profession, the operation of equipment, and techniques. After many centuries, the profession of property surveying is having a renaissance of spirit. Recent years have seen, in the professional literature of the United States, more professional cooperation among property surveyors, and more original thought applied to the problems of the profession, than has any prior period.

The task of the property surveyor has been defined as the engineering interpretation, definition, and representation of legal documents of land ownership, a task in which the tools of the engineer, his mathematical knowledge, and his capacity for analytical and logical thinking are applied to the problem of reducing to tangible and explicit form the legal records of land titles.

The failure on the part of the lay public to appreciate the true function and importance of the property surveyor has long concerned the profession. Far greater damage to the profession, however, results from the failure of the engineering profession itself to appreciate the true nature of the responsibilities of the property surveyor. The causes of those failures are many. In a large measure, years of apathy within the profession itself form one cause. Another is the almost total lack of property surveying instruction in engineering colleges.

[4] "A Treatise on the Law of Property," by William F. Walsh, Baker, Voorhis and Co., New York, N. Y., 2d Ed. 1937, p. 742.

[5] Cleveland, Ohio.

It is the present problem of the profession to educate its own members to their responsibilities and importance, and then the engineering profession and the lay public.

Mr. Wattles' main point, the primary importance of the legal title, is recognized, of course, as basic in all accepted current thought on the subject. The "Technical Standards for Property Surveys," [6] compiled and adopted by the American Congress on Surveying and Mapping in 1946, begins with the statement:

> "Each parcel of land whose boundaries are surveyed by a licensed surveyor, should be made conformable with the record title boundaries of such land."

Nevertheless, this statement still comes as a shock to many engineers and some property surveyors, to whom the old rule, "occupation controls," has almost Biblical significance. There are many reasons for the development of the "occupation controls" rule. Very important among these reasons is the fact that, since the beginning of the twentieth century, much of the responsibility of land description has been left to nontechnical "scriveners." These men are responsible for the prevalence of such forms of descriptions as:

> "My house and the land upon which it stands."

or

> "Bounded on the west by the State Road; on the north by the land now or formerly owned or claimed by Mary McNamara; on the east by land now or formerly owned by Agnes Beebe; and on the south by the land of The Harbor Building Company."

Since such descriptions are entirely lacking in specific definition, the surveyor's only recourse is the use of occupation.

An additional factor in the development of the "occupation controls" rule is the fact that the occupation survey is so often the easiest solution, and is welcomed by the poorly qualified surveyor, to whom the law and titles are likely to be something unknown and mysterious. It is also often the solution preferred by the client, whether proper or not, since it conforms to his own assumptions as to his ownership.

The erroneous idea that adverse possession over the statutory period automatically grants title also adds to the misconception. Modern title practice, dealing as it does with the recorded document, rather than the property itself, must of necessity pin its responsibility to the written deed. As a result, the interpretation of the deed by the surveyor becomes an increasingly meticulous process, in which the title becomes the framework on which the surveyor hangs his findings of occupation, possession, adjoiners, etc. Modern high land values, relatively small parcels of land, and extensive improvements of land, particularly in urban areas, tend to make the deed and the precise wording of the description of greater significance than in the past. It is inevitable that, in a growing community, the title as recorded must be recognized as the controlling factor in land ownership.

[6] "Technical Standards for Property Surveys," Am. Cong. on Surveying and Mapping, Washington, D. C., 1946.

The property survey, then, becomes an exposition of all pertinent facts relative to the record title (those in agreement as well as those not) rather than a determination of ownership. The surveyor becomes a fact finder, rather than a judge of ownership. His recommendations and opinions, nevertheless, have extrajudicial value in direct ratio to his reputation for thoroughness, knowledge, and judgment.

Mr. Wattles is to be commended on a particularly clear statement of a too little recognized fact. It is to be hoped that his paper will be widely read and digested by both practitioners of property surveying and by those civil engineers who, in the past, have not always viewed the practice of property surveying with sufficient seriousness.

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DISCUSSIONS

CITIES CANNOT LIVE WITHOUT TRUCKS

Discussion

BY JOHN A. MILLER, AND L. A. NEES

JOHN A. MILLER,[11] M. ASCE.—In what appears to be a blanket argument against any restrictions on the operation of intercity motor trucks within city limits, the author presents certain statements as basic facts which, to the writer, seem to need supporting evidence. The first such statement (see heading, "Fallacies in Thinking About Trucks") is that: "* * * large American communities could not exist without modern truck transportation." However, the City of New York (N.Y.) had a population of 4,766,883 in 1910 when there was no modern truck transportation. At this same date a considerable number of other American communities could also properly be classified as large. None of them found it impossible to "exist"; nor would large communities today find it impossible to dispense with motor trucks, although they would be considerably inconvenienced, particularly if deprived of the service of small city-delivery trucks.

This distinction between "convenient" and "essential" is extremely important. The expenditure of huge sums of public money may be justified to create improved facilities for something essential to existence when it would not always be justified to create facilities for something that is only a convenience and an advantage.

A second statement (also under the heading, "Fallacies in Thinking About Trucks") is made that: "* * * the volume of truck freight is so large and so important that it must be moved continuously and during the regular working hours of business establishments." Presumably Mr. Stevens means that no restriction should be placed on the hours during which intercity trucks may move within the city. Since the maximum spread of regular working hours of business establishments in any city would not extend beyond the 12-hour period from 7 a.m. to 7 p.m., there remains another 12-hour period from 7 p.m. to 7 a.m. outside regular working hours. When the author states that the volume of heavy truck transportation is so large that it must be operated in city streets during the daytime as well as during the night (when other traffic move-

NOTE.—This paper by Hoy Stevens was published in December, 1948, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: April, 1949, by Harold M. Lewis.

[11] Schenectady, N. Y.

ment is at a minimum), it is equivalent to declaring that the virtually unrestricted use of roadway facilities for half of the twenty-four hours is insufficient to accommodate all the truck traffic. The writer has never seen any statistics to show that this theory is true, and he suspects that the motor truck operators would be the first to deny its truth if they were asked to pay taxes on that basis. It seems probable, therefore, that the real basis of the author's objection to restrictions on daytime operation is that such restrictions would increase the cost of truck operation rather than that facilities are inadequate.

A third statement (again under the heading, "Fallacies in Thinking About Trucks") is made that: "Certainly, substituting 2 or 3 small vehicles for one large vehicle will not improve traffic situations." To dispute the certainty of that view, the author's own data show that only 37% of the tonnage at Chicago, Ill., was handled in unit, full truck loads. When delivering the remaining 63% of the tonnage, the trucks were running partly empty most of the time. Wherever traffic congestion exists, a partly loaded truck represents a waste of valuable street space. Under such circumstances the use of smaller trucks would be a definite advantage.

The most serious objection to the use of extremely large trucks is the congestion they cause when making turning movements in congested areas and when stopped at an angle to the curb. In such situations there can be no doubt that 1 large truck does more to create congestion than do 2 or 3 small trucks. This does not necessarily mean that the use of large trucks is never justified, but it does mean that the use of smaller trucks is desirable under many circumstances.

The author seems to conclude that it is the duty of the community to provide such facilities that the truck operator can operate whatever kind of vehicle he wants in whatever way he wants to operate it. As an ordinary citizen and taxpayer in the community, the writer rejects the contention that the community has any such duty.

L. A. NEES,[12] Assoc. M. ASCE.—This thought-provoking paper touches on a feature of city planning and building design to which in great degree, insufficient attention is given, because of either lack of knowledge of the problem, or the barrier of individual or public opinion. Off-street truck loading takes considerable area and may entail a serious encroachment on the space available for the activity planned for the building. It is the duty of the engineer or architect, in designing a building, to give the client a structure that will serve his purpose efficiently at the lowest possible cost. Except in cases where legal provisions exist or where the loading and unloading of freight are major items in the facility, the client is likely to regard these facilities as a necessary evil, and to view with a jaundiced eye any designs that add considerably to the cost of the structure, or infringe on what he pleases to call his "operating space." This reaction is especially true of smaller industries or businesses where the volume of truck freight to be moved is not large. Therefore, the designer is faced with the problem of convincing his client of the "indirect benefits" involved in adequate loading facilities, especially where they encroach measurably

[12] Headquarters Air Materiel Command, Wright-Patterson Air Force Base, Dayton, Ohio.

on the available building space. Quite frequently the study and analysis required to demonstrate this fact are beyond the scope of his organization, or his fee, or both.

It appears then that municipal control in the form of codes or regulations is the logical basic approach to the problem. In establishing such codes, a city is faced with two fundamental problems: (*a*) The establishment of requirements for construction in areas as yet undeveloped; and (*b*) the establishment of requirements for construction in existing developed areas. Item (*a*) offers full latitude for the development of straightforward long-range planning. The solution for item (*b*), however, calls for bold and imaginative planning, coupled with an intensive educational campaign to convince the taxpayer that such measures eventually increase the economic health of the community.

The writer suggests that the first steps in the development of a "code" are to zone the city into districts and to study the type of truck freight serving each zone. Such a study should aim at determining the average daily volume of truck loadings and unloadings for a range of truck capacities based on size. It is anticipated that this research would yield a truck size that could be considered average for each zone, together with a clue to the average density of loadings per enterprise, critical street, or zone.

From such data, combined with traffic counts, and growth-of-use forecasts, the planner would be in a position to weigh the necessity of thoroughfare widening in the zone, since this aspect is intimately related to the problem of developing a code for off-street loading facilities.

Based on such studies, a code would contain the following provisions:

1. Establishment of revised property lines as required, for all new construction;
2. Establishment of minimum off-street loading facilities defined by "setback" distance from the street line, width of unloading space in either feet or number of berths, turnout radii to the loading area, etc;
3. Requirement that the construction plans be submitted for approval to an appropriate engineering representative of the city, together with an engineering analysis of anticipated volume and intensity of freight to be handled and types of trucks expected to serve the facility, as a justification of the loading facilities proposed for construction; and
4. A time limit in years at the expiration of which all existing structures must be modified to conform to the code.

It is apparent that the optimum degree of development in any zone is that stage at which the interrelation between structure, truck sizes, and width and type of the traffic artery is such that the total combined annual charges for all phases is a minimum. Achievement of this goal necessitates the introduction into the code of a restriction on the maximum size of truck that may be permitted to deliver freight in any one zone. There is a limit as to how far a city can go in designing its traffic arteries to accommodate larger and larger trucks. It appears that, in planning, a point can and must be reached within which trucking industries can balance their fleets economically to conform to established reasonable zoning restrictions.

Enterprises of like character, and therefore with similar basic freight requirements, have a habit of choosing the same environment. Such development can be observed in many of the larger cities. There is a strong possibility, therefore, that the studies outlined herein will indicate that a great percentage of the freight in any zone is handled within a certain limiting truck size. If such an economic grouping exists, there is no reason why it should be paralyzed in its development by codes that impose restrictions on structures but at the same time permit unrestricted delivery by trucks that are oversized, not only for the type and the volume of freight they deliver, but also for the streets upon which they travel. The writer has observed frequent instances of large tractor trailers delivering small parts of their loads to consignees. It has not been possible to judge the extent of this practice, but its economics for intracity deliveries are seriously open to question except where required by the dimensional character of the freight.

In the future development of a city and its trucking industries, this concept therefore implies a greater development of major "in transit" and "distribution" freight terminals. It implies also that trucking industries will be forced to scrutinize carefully the composition of their truck fleets with a view to determining economic sizes and achieving a proper relation in numbers. It is possible that a thorough study of the problem along the lines indicated for any actual case will demonstrate the validity of a plan leading to uniform development within the zones of a city.

The author's contention that criteria based solely on a general type of enterprise or space values are questionable guides is a fair one. The volume and the character of freight to be moved and the intensity of movement are more reliable indexes.

The statement that the dock area must be at least twice the total body floor area of the largest number of trucks that can be docked at one time is open to question. Although it may be statistically correct in many instances, this requirement may prove too rigorous. For instance, the writer has seen numerous cases of government warehouses which do not meet this criterion where the freight never stays on the dock but is handled rapidly and expeditiously by fork-lift equipment which stocks it in its proper place without rehandling. This procedure is especially applicable where the cargo is all of one type. The dock space requirements are intimately related to the character and the method of storage in the structure. Its design, therefore, must represent an integrated step in the space planning for the entire facility.

The author states in his conclusions that trucking associations are conducting studies on this problem. The writer feels that such studies will be of great value to the engineering profession and looks forward to their publication.

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DISCUSSIONS

DEVELOPMENT OF CBR FLEXIBLE PAVEMENT DESIGN METHOD FOR AIRFIELDS A SYMPOSIUM

Discussion

BY MILES S. KERSTEN, AND F. C. TYRRELL AND L. A. PALMER

MILES S. KERSTEN,[42] ASSOC. M. ASCE.—This Symposium has presented in a clear manner explanations of many of the features of the California Bearing Ratio (CBR) method of design of flexible pavements. The following discussion concerns just two points.

Extensive field testing and observations indicate that there is good agreement, for the most part, between the thickness requirements as given by the design curves and the pavement thicknesses giving satisfactory service. It appears that this agreement has been obtained by using the field, or in-place CBR test, results. The question arises as to how good an agreement would be obtained if laboratory CBR test values were used. Another way of posing this question would be to ask how much agreement is there between CBR values obtained on soaked laboratory samples and those on subgrades on which pavements have been in place for a long period of time.

Correlation between the laboratory and field tests is dependent on both moisture content and other factors such as soil structure and methods of compaction. It is the opinion of some engineers that laboratory bearing tests on soaked specimens always give smaller CBR values than tests on the prototype. This thought is probably based on their concept of the soaking procedure and the resultant high moisture content. However, it may be that such factors as inadequate field compaction, required speed in field construction, and differences in soil structure may quite often tend to give lower field CBR values for a subgrade than for the soaked laboratory specimen. Paper No. 4 by Messrs. Jervis and Eustis indicates that considerable work has been done to develop the laboratory procedure so that the results are applicable to field conditions. Additional data or comment as to how successful this has been would be enlightening.

NOTE.—This Symposium was published in January, 1949, *Proceedings*.

[42] Associate Prof., Civ. Eng., Univ. of Minnesota, Minneapolis, Minn.

Messrs. McFadden and Pringle, in paper No. 1, and Mr. Turnbull, in paper No. 11, mentioned studies in which data on subgrade moisture conditions are being collected. Some comment on the findings made thus far in such studies would be welcomed. There have been numerous general comments elsewhere on extremely wet subgrades being encountered in arid regions. However, very little actual data of such a nature are to be found. The writer was connected with some studies of subgrade moisture conditions made by the Highway Research Board of the National Research Council.[43,44] These studies indicated that for subgrades in arid or semiarid regions the moisture contents were markedly less than those in humid regions. For example, the data for forty-three airfields in western Texas, New Mexico, Arizona, and in parts of California with less than 11-in. annual precipitation show that less than 10% of the subgrade soil samples had moisture contents as great as their plastic limit. This was true even for clay soils which normally tend to exist at high moisture contents. This study was for airfields which, in many cases, had been paved for only a year or two. A test program sponsored by the Los Angeles (Calif.) District, and conducted with the cooperation of the Arizona and Nevada highway departments, consisted of taking samples of the subgrade at from three to six locations on the highways adjacent to, or as close as possible to, airfields. The pavements studied had been in place from 2 years to 30 years; the average age was about 12 years. The soils encountered were for the most part quite sandy. Of the ninety-three samples reported with plastic limits, only two had moisture contents in excess of this constant. Even the heavier textured soils (of which there were only about a dozen) did not tend to be wetter than their plastic limit. Studies in more humid sections of the United States indicated a much greater proportion of tests attaining high moisture contents.

If the test results of the current study of the Corps of Engineers, Department of the Army, indicate that moisture contents do tend to remain low beneath pavements in arid or semiarid regions it may be possible to alter the CBR procedure for soils from such areas. Some comment by the Symposium authors on the trend of their results would be of value.

F. C. Tyrrell,[45] Assoc. M. ASCE, and L. A. Palmer.[46] —The eleven papers of the Symposium present, in concise form, the results of several years of research along a broad front in the comparatively new field of airport pavement design. Each paper is a definite contribution in itself and the entire group of papers has served very materially to advance the profession. Many readers will recognize the fact, however, that the information contained in this Symposium is actually but a small part of the total which the authors have made available and which deals with both classes of pavements, flexible and concrete.

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[43] "Survey of Subgrade Moisture Conditions," *Proceedings*, Highway Research Board, National Research Council, Vol. 24, 1944.

[44] "Subgrade Moisture Conditions Beneath Airport Pavements," *ibid.*, Vol. 25, 1945.

[45] Comdr., CEC, U. S. Navy, Director, Eng. & Technical Services Div., Bureau of Yards and Docks, Dept. of the Navy, Washington, D. C.

[46] Chf., Soil Mechanics and Paving Branch, Bureau of Yards and Docks, Dept. of the Navy, Washington, D. C.

For an organization that has several well equipped laboratories, stationed in different sections of the United States, there is a considerable advantage in using a type of test such as the North Dakota cone and the California Bearing Ratio (CBR) test. Because of their simplicity and the speed at which such tests can be made, it is possible to obtain, expeditiously, by their use, a thorough survey of soils and available aggregates in any locality. In the final analysis, however, a pavement and the supporting earth under it must be considered as a structure. What goes into the structure is important but the procedure whereby a material becomes an integral part of the structure—that is, the workmanship or construction—is a factor of equal importance. Any test that is a test of the finished product, the structure itself, reflects the combined influence of all the factors, including both materials and construction. Paving is a case of mass production and in all cases of mass production the final criterion is a test of the finished product. This may be in the form of actual traffic tests on pavement strips built especially for testing purposes or it may involve static load tests on several trial pavement sections constructed within the area of a proposed runway, parking area, etc., that is to be paved. The construction of trial pavement sections has proved to be in itself (and apart from load tests made on such sections) a most valuable source of practical information. By this method it has been found, in Navy experience at least, that certain difficulties encountered in the field compaction of certain subbase and base course materials could not have been predictable from the laboratory compaction tests.

It is no doubt possible to establish an over-all statistical relationship between flexible pavement thickness and a laboratory test, such as the CBR test. Such statistical realtionships are exemplified by Fig. 7 in the paper by Mr. Middlebrooks. Such statistical curves may be improved and refined almost endlessly by obtaining more and more correlative data with respect to actual service performance. However, in any graphical representation of this kind the actual scattering of the points representing individual sections of pavements, runways, taxiways, or even entire airfields that do not receive special study cannot be shown. The exceptions that would fall on the unsafe side of any such curvilinear relationship, plotted from a great collection of statistical data, could be very costly. The general tendency, therefore, in all such statistical procedures, is to preclude exceptions that could be on the unsafe side by overconservatism in establishing the thickness design curves. This fact should be appreciated and thoroughly borne in mind by all who design flexible type pavements. The fact that such statistical relationships have considerable utility should also be appreciated since they tend generally to establish the upper limits in the design of pavement thickness.

For any specific case one cannot estimate the actual factor of safety by using the thickness versus the CBR values. A substantial factor of safety is required in a runway where sudden pavement failure may be hazardous to costly planes and human life. On taxiways and parking areas pavement failure usually involves nothing more than local patching of a paved area. Nevertheless, by reason of dynamic loading, taxiways, warm-up aprons, and

parking areas are subjected to extra heavy loading. A factor of safety of 1.75 against overstressing of concrete pavement seems to be accepted generally as ample for unlimited operations. It is reasonable to suppose that the factor of safety against overloading a flexible pavement need not be more than this amount. The real job of engineering is to meet both requirements—safety and economy—in airport paving.

The Bureau of Yards and Docks, in the United States Department of the Navy, has considered that these requirements have demanded a realistic approach to the problem of flexible pavement design, that generalities should be avoided to a considerable extent, and that it is not economical to rely too much on statistical data and procedures in designing a flexible type of pavement for an individual airfield. It has concluded further, and after extensive field studies, that the assumption that the subgrade under the pavement, in time, will become as soft from the infiltration of water as the compacted laboratory samples subjected to 4 days of soaking, is absolutely untenable. The bureau has become increasingly aware also of a considerable degree of variation in the load-distributing properties of different types of subbase and base course materials and aggregates, a consideration that hitherto has been ruled out in using the CBR design yardstick. The ruling out of this factor was admittedly an error according to the observation recorded in the paper by Mr. Turnbull (under the heading, "The Design Curves"). This observation is a definite advance in the improvement and the use of the CBR design procedure. The use of design curves obtained by plotting concrete pavement thickness versus either the subgrade reaction modulus, or even the subgrade CBR, might seem to be warranted since concrete is a relatively standard material; but the use of such curves for any and all kinds of flexible pavements consisting of diverse types of subbases, base courses, and surfacings, irrespective of the extent to which each of the several layered components spreads the load, seems not to be warranted. Modification of the CBR thickness design curves in a manner such that they reflect the diverse load-spreading characteristics of different materials and of materials compacted differently is one of the next needed improvements in the procedure.

Another needed modification of the CBR design yardstick, according to the experience of the Bureau of Yards and Docks, is considerably less conservatism in estimating or anticipating the degree of softening or of loss of bearing value of the subgrade as caused by infiltration of surface water. The bureau has reported[47,48] the subgrade data obtained at thirty-two Naval and Marine Corps air stations, in various sections of the United States. The in-place moisture content and densities of the subgrades under the pavements were obtained in each case by Navy field personnel assigned to this task. The pavements had been subjected to traffic for variable periods of from 2 years to 6 years. The average in-place moisture content of the top 9 in. of subgrade did

[47] "Pavement Evaluation by Loading Tests at Naval and Marine Corps Air Stations," by L. A. Palmer and James B. Thompson, *Proceedings*, 2d International Conference on Soil Mechanics and Foundation Eng., Rotterdam, June, 1948, Vol. II, pp. 222 to 236.

[48] "Field Loading Tests for the Evaluation of the Wheel Load Capacities of Airport Pavements," by L. A. Palmer, in "Symposium on Load Tests of Bearing Capacity of Soils," *Special Technical Publication No. 79*, A.S.T.M., 1947.

not exceed the average laboratory-determined optimum at seventeen airfields out of a total of twenty-six airfields for which laboratory optimum moisture data were available. At only one field was the average subgrade moisture above the average plastic limit of the soil and at this field the surfacing was in a very bad condition at the time of testing. At five out of twenty-seven fields having flexible type pavements the average in-place subgrade dry density exceeded the maximum laboratory-compacted dry density. This observation is in good agreement with the conclusions in paper No. 8 by Mr. Redus. A study of the tabulated data reported by the Bureau of Yards and Docks discloses very few individual instances where the maximum moisture content of the subgrade was above the plastic limit. The general trend was clear. There seemed generally to be a progressive diminution in moisture and a corresponding increase in subgrade density as traffic was continued and the pavements were kept in a reasonably good state of repair. Exceptions to this trend included pavements where the maximum ground-water level was less than 2 ft below the pavement. Similar subgrade and moisture data have been reported for Canadian airfields by Norman W. McLeod,[49,50] who further presents a direct comparison between the CBR wheel load rating and the actual traffic sustained by four Canadian airports. As an example of this comparison, Dorval Field at Montreal, Que., Canada, without excessive maintenance cost, sustained 23,000 operations of planes weighing more than 64,000 lb between January 1, 1942, and January 31, 1947, with a CBR load rating of 5,000 lb for a single tire. Quoting from Mr. McLeod's closure,[49]

> * * * "Canadian experience indicates that an airplane wheel load of any given magnitude can be carried by a pavement and base course which have only a fraction of the thickness specified by U.S.E.D. [United States Engineer Department] design."

The experience of the U. S. Navy is in 100% agreement with this statement. At the present time (1949) the pavements at sixty of its air stations have been studied and evaluated. These sixty air stations, with few exceptions, have flexible type pavements of from 6 in. to 12 in. in total thickness and, at comparatively low maintenance cost generally, they have sustained the traffic of planes having single wheel loads of from 5,000 lb to 60,000 lb. For the subgrades at these same fields and on the basis of the CBR design curves, the pavement thicknesses should have been from 12 in. to about 45 in. in thickness to have sustained this same traffic.

Fortunately, for the most part, overdesign by the CBR and other design yardsticks has not been too extravagant. It is understood that by taking the CBR equipment to the field in use and obtaining the in-place CBR values of the subgrades and base courses the wheel load rating of the pavement may be increased over its earlier design bearing value. This is a more realistic approach and in effect is a correction of the initial design load estimate. How-

[49] "Airport Runway Evaluation in Canada," by Norman W. McLeod, *Research Report No. 4B*, Research Correlation Service, Highway Research Board, National Research Council, 1947.

[50] "A Canadian Investigation of Load Testing Applied to Pavement Design," by Norman W. McLeod, in "Symposium on Load Tests of Bearing Capacity of Soils," *Special Technical Publication No. 79*, A.S.T.M., 1947.

ever, since a field CBR test is essentially a miniature loading test, the question arises as to why it would not be preferable to make actual loading tests on areas comparable to those of the tire prints? According to James B. Newman, Jr.,[51] M. ASCE, plate bearing tests have a definite place in the determination of strengths of completed pavements. The Bureau of Yards and Docks has found plate loading tests to be expedient and reliable both in the evaluation of flexible pavements after construction and in the design of new pavements. On the average, the loading test data are in good agreement with theory, but because of the exceptions no theory is relied on in bureau procedure. The wheel load rating of the runway, taxiway, etc., is made contingent on the data obtained from the weakest spots in the paved area, with the stipulation that such weak spots be truly representative of the entire paved section.

Traffic tests, either on actual pavements or on specially constructed test strips, should simulate actual and not exaggerated traffic conditions. In the Stockton Field tests in California, described in Mr. Porter's paper, 1,600 repetitions of a moving 40,000-lb wheel load "in one rut" would seem to give a quite distorted picture of the effect of normally distributed traffic. Even on the narrowest of taxiways there never is "tracking" to this extent. One can imagine the extreme punishment taken by a flexible pavement when 3,730 vehicles, each producing 40,000 lb of pressure on single tires, line up, one behind the other with all the tire loads traversing paths that are only three times the width of a tire. Field studies by the Bureau of Yards and Docks show definitely that traffic compacts the base and subgrade generally and under the entire taxiway and runway. This is compaction rather than shear and the data reported by Mr. Redus seem to be in good agreement with the Navy field studies. Mr. Redus reports progressive consolidation of both plastic and non-plastic base courses by traffic. Progressive base and subgrade compaction, all in one rut or narrow path, will inevitably cause failure of the bituminous surfacing which cannot act as a beam.

Finally, a comment on the CBR test itself is required. The standard loads of 1,000 lb per sq in. and 1,500 lb per sq in. for 0.1-in. and 0.2-in. penetration, respectively, have reference to a "standard crushed stone," as described in Mr. Porter's paper. Aside from the fact mentioned by Mr. Turnbull that "large particles directly under the piston affect the results," it is obvious that the type of deformation that characterizes the standard crushed stone is vastly different from that which is characteristic of soaked, plastic clays, and silts. There can be no "modulus of shearing resistance" that is commonly characteristic of both of these extremely different types of materials, because the top inch of soaked clay is practically at its liquid limit and the penetration is due to displacement without either elastic compression or appreciable consolidation. On the other hand, the standard crushed stone is a brittle material that possibly fractures to some extent and surely must undergo elastic compression under the load of the piston. Again, variations of as much as 1,000% in "corrected" CBR values have been reported for the same soil at the same moisture content

[51] Discussion by James B. Newman, Jr., of "Military Airfields: A Symposium," *Transactions*, ASCE, Vol. 110, 1945, pp. 734–737.

by the Vicksburg (Miss.) laboratory,[52] the variations being attributed primarily to different compaction procedures. A carefully controlled standard compaction procedure will eliminate all such extreme variations in measured CBR values, but is such a laboratory standard truly indicative of the actual field compaction? The summary of Mr. Turnbull's paper seems to express a reasonable doubt as to this possibility under diverse field conditions.

In conclusion, it is believed that although great progress has been made in using the empirical CBR design method, pending still further needed modifications of this method, more realistic procedures (such as direct loading tests or traffic tests that simulate actual traffic conditions) are required for the sake of economy in pavement design.

[52] "The California Bearing Ratio Test As Applied to the Design of Flexible Pavements for Airports," *Technical Memorandum No. 213–1*, U. S. Waterways Experiment Station, Vicksburg, Miss., 1 July 1945, plate 109.

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DISCUSSIONS

MISSISSIPPI RIVER BRIDGE AT DUBUQUE, IOWA

Discussion

BY LOUIS BALOG

LOUIS BALOG.[17]—The layout, floor design, and bracing arrangement offered in this paper, viewed in the light of progressive developments in bridge design, invite comment on features that the writer proposes to outline herein.

Economy.—In the "Synopsis," the authors state:

> "In economy it [the Julien Dubuque Bridge] compared favorably with cantilever, tied arch, and suspension span layouts studied for the crossing."

The cantilever, being a statically determinate system, cannot utilize the tenacity and ductility of the steel; and the simple tied arch, if its features are identical with those of the bridge as built, must necessarily cost more than the continuous truss. In deciding the relative economy of a suspension span layout it is helpful to compare the Davenport (Iowa) suspension bridge (1935)[18] with the Dubuque project, since they are similar in size and clearance requirements.

The Davenport crossing is located at Bettendorf, Iowa, 80 miles south of the Dubuque bridge. Table 6 demonstrates the degree of similarity. The ratio of the substructure cost to superstructure cost is the same in both cases (line 9, Table 6). The Dubuque spans, however, cost 62% more per square foot of width between handrails (line 10, Table 6) than did the Davenport spans despite the fact that the Davenport approaches contain six 223-ft, and three 162-ft, truss spans. This economic superiority of the Davenport bridge is due mainly to its suspension span which contains about one third as much steel as the Dubuque truss span. Its floor system and stiffening trusses, respectively, contain 380 tons and 480 tons of steel in contrast to 600 tons of steel, for the bracing alone, in the Dubuque truss span. Similarly, extreme comparative economy in metal of suspension spans is demonstrated by the metal content of three other existing suspension bridges of comparable size.

NOTE.—This paper by R. N. Bergendorff and Josef Sorkin was published in June, 1947, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: September, 1947, by Jonathan Jones; October, 1947, by A. Floris, and T. H. Rust; and May, 1948, by Charles W. Dohn.

[17] Cons. Engr., Binghamton, N. Y.

[18] "Building the Davenport Bridge Across the Mississippi," *Engineering News-Record*, Vol. 115, 1935, pp. 837–841.

Bridges of the continuous and stiffened continuous types of advanced design, as indicated by alternate designs prepared by the writer for the Dubuque truss span, contain 20% less metal than the bridge as built.

Layout.—Lowering the mass centroid of a body increases its stability. The application of this principle in the distribution of the mass of a bridge minimizes wind and dynamic effects; and, generally, it produces an economical structure, pleasing in appearance. Considerations of statics, stability,

TABLE 6.—COMPARATIVE ECONOMIC DATA, DAVENPORT AND DUBUQUE BRIDGE PROJECTS

Line (1)	Description (2)	Davenport (3)	Dubuque (4)
1	Length of open structure, in feet	4,955	5,759
	River Crossing:		
2	Type	Suspension	Curved truss[a]
3	Span length, in feet	370+740+370	345.75+845.17+345.75
4	Total span length, in feet	1,480	1,536.67
5	Width between handrails, in feet	25.92	31.92
	Costs in Dollars:		
6	Substructures	357,601	832,000
7	Superstructures	686,798	1,588,000
8	Total, items 6 and 7	1,044,399[b]	2,420,000
9	Ratio, line 6/line 7	0.52	0.52
10	Unit cost of spans, in dollars per square foot of deck[c]	8.13[b]	13.17

[a] Continuous curved truss, with tie. [b] Includes cost of toll plaza at the south end of the suspension span. [c] Width between handrails.

economy, and esthetics require that parts of the bridge above the roadway be as light, and that this area have surfaces as small, as possible. The bridge should direct instantaneous attention to its cardinal purpose—a continuous road crossing a waterway. Consistent with this ideal, the designer's central objective is to lead all loads to the ground in the most natural manner.

If one of the spans of the continuous girder is twice (or more than twice) as long as the others, the required stiffness of the girder can be achieved by

1. Adding a system of stiffening members; and
2. Shaping the girder so that its form assures stiffness.

The latter effect can be accomplished either by

a. Basing the depth of the layout on the stiffness requirement of the long span; or

b. Arching the girder in the long span and introducing a tie for the horizontal reactions.

Types 1 and 2*a* are natural solutions of the problem, satisfying the foregoing basic requirements. In the writer's opinion, type 2*b*, in general, and the Dubuque truss span,[19] in particular, do not satisfy these requirements.

Study of ninety-one bridges of type 1, built and proposed, lead the writer to believe that efficient and pleasing structures result from this type, in which the stiffening system of the shallow girder can be either a compression or a

[19] *Construction Methods*, March, 1943, photograph on p. 47.

tension chord, Figs. 10(*a*) and 10(*b*). Such stiffening chords may be curved, trapezoidal, or triangular in elevation and need not extend from support to support. The small wind load and other horizontal forces, received by these stiffening chords, make possible the design of pleasing overhead bracing, which assures, also, the buckling stability of these chords in the horizontal plane. Their stability in the vertical plane is governed by the spacing of the suspenders. Tension chords need not be braced. As a result of the loads acting on the girder, and of the weight of the chords themselves, such chords transmit

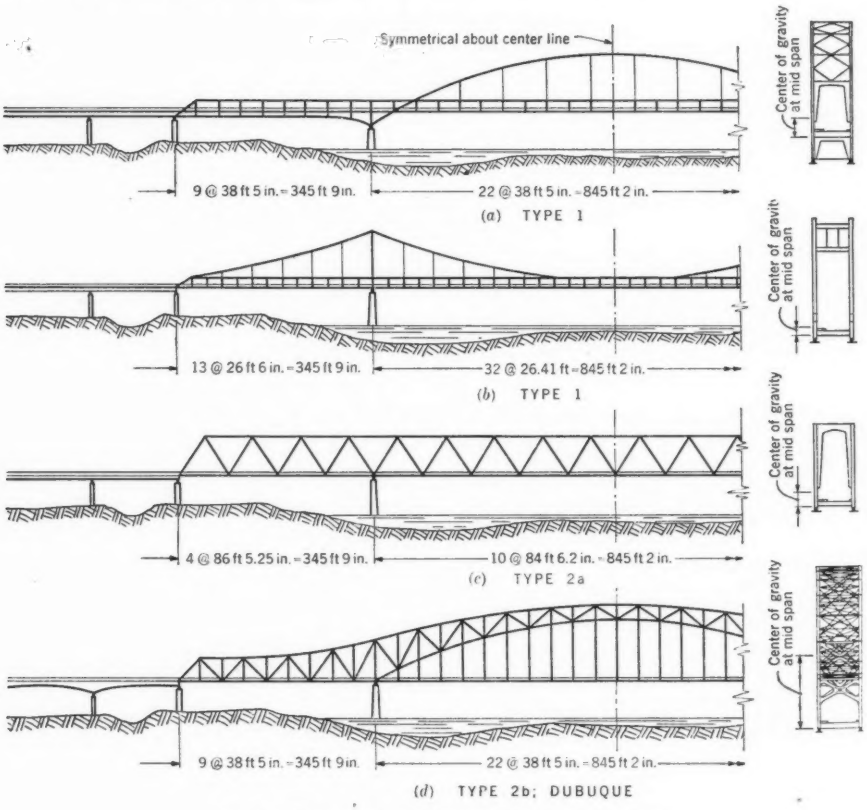

Symmetrical about center line
Center of gravity at mid span
9 @ 38 ft 5 in. = 345 ft 9 in.
22 @ 38 ft 5 in. = 845 ft 2 in.
(a) TYPE 1
13 @ 26 ft 6 in. = 345 ft 9 in.
32 @ 26.41 ft = 845 ft 2 in.
(b) TYPE 1
4 @ 86 ft 5.25 in. = 345 ft 9 in.
10 @ 84 ft 6.2 in. = 845 ft 2 in.
(c) TYPE 2a
9 @ 38 ft 5 in. = 345 ft 9 in.
22 @ 38 ft 5 in. = 845 ft 2 in.
(d) TYPE 2b; DUBUQUE

FIG. 10.—ELEVATIONS OF VARIOUS TYPES OF TRUSSES

horizontal forces at their points of attachment to the girder and therefore decrease the deflections of the long span to satisfactory values. A great variety of combinations of girder and chord properties are possible in which the magnitude of the horizontal forces can be regulated primarily by the geometry of these chords.

Type 2*a* (Fig. 10(*c*)) may have constant depth; it may have the greatest depth at midbridge, or over the intermediate supports. In the latter case the truss can be shaped so that, except for the two portals over the intermediate piers, no other visible bracing is necessary. Examples of all these forms exist.

The forms of type 2*b* (Fig. 10(*d*)) may vary between two extremes: The truss may be shallow and of nearly constant depth (except in the panels adjacent to the intermediate piers, where it is extended down below the floor to join the deck side spans), or it may be deep and an entirely through span, with greatly varying depth. The Dubuque truss span represents the latter extreme, in both truss configuration and bracing arrangement. Arching of the center span truss in this case directs attention 120 ft above the roadway to a confusion of main members and bracing members, many times as deep as the floor,[19] and raises the center of gravity from 25 ft above the bottom of the steel at piers 18 and 19, to more than 60 ft at midspan, thus decreasing the stability of the structure. The normal and vibratory effects of wind, impact, and possible earthquake forces will be greater in this bridge (since the center of gravity is high above the floor) than in bridges of types 1 and 2*a*. The centers of gravity of the latter bridges (see Fig. 10) are low and their areas exposed to wind are smaller and closer to the roadway level. In the Dubuque truss span large structural areas, high above the floor, receive considerable wind loading, that is directed down to the supports by a multitude of crisscrossing braces. In driving across this bridge, an engineer, comparing the effect of the view of this bracing with that of the lack of the truss diagonals in the center panels, will not agree to the observation in the "Synopsis" that:

> "The three-span continuous truss structure * * * presents a pleasing appearance, affords to traffic a view unobstructed by diagonal truss members across the 845-ft main channel span, * * * "

Unlike subdiagonals and an almost solid aspect of braces, main diagonals are not objectionable. They can be avoided economically, however, by the use of stiffened plate sections and welded details. Figs. 10(*a*) and 10(*b*) show such layouts. Fig. 10(*c*) shows a layout in which an unobstructed view is provided for five cars in a row (the authors providing only for two) by a continuous triangular system composed of a few, large, box shaped, stiffened plate members. The bottom chord is a box girder 8.5 ft deep. The relative depth of the top and bottom chords emphasizes the roadway and thus expresses the purpose of the structure. The properties of the bottom chord assure great savings in floor metal, handrails, and gussets; simple bracing, few joints, and reduced dead load contribute to the economy of this bridge—it contains 860 tons less metal than the bridge as built. This design demonstrates that the inclination of the members in a truss is less important than their number in avoiding both obstructions to view from the bridge and the maze of intersecting members in looking at the bridge.[20]

Adequate traffic capacity constitutes the basic requirement for the width of a bridge. The authors, however, discuss additional considerations. Under the heading, "Three-Span Through Truss Continuous Unit," they state:

> "A saving of floor beam metal would have resulted from a closer spacing of trusses, with sidewalk outside; but truss loadings would have so differed that the trusses would not have been identical and duplication of fabrication would have been sacrificed. Also, closer truss spacing would have reduced what was regarded as a desirable width-length ratio for lateral stability."

[20] *Construction Methods*, March, 1943, photograph on p. 114.

A narrower bridge may contain considerably less metal than that saved in floor beams alone, and the sacrifice of duplicate fabrication of the trusses by a sidewalk outside can be minimized by proper design to a negligible amount. The statement that lateral stability required the width used is not borne out by (*a*) the behavior of existing bridges, and (*b*) the progressive developments in bridge design since 1918.

(*a*) Lateral stability of a truss bridge is governed mainly by its torsional stiffness and the height of its center of gravity at midspan. The torsional loadings on the Dubuque truss span are much smaller than, for instance, those on the Mississippi River bridges at Memphis, Tenn. (1913), New Orleans, La. (1935), and Baton Rouge, La. (1940), which carry railway loads between, and highway loads outside, their trusses. The span lengths of these latter bridges are comparable or larger, and their unbalanced highway loads alone equal the total live load of the Dubuque truss span, in addition to which two of them carry an unbalanced railway loading. The twisting moment per linear foot acting on the Baton Rouge bridge is more than three times as large as that acting on the Dubuque truss span, yet a 3-ft-smaller width was found to be satisfactory at Baton Rouge despite its greater span length. Furthermore, a large number of truss bridges having width-length ratios up to 40% smaller than the width-length ratio of the Dubuque truss span proved themselves laterally stable for many years in service. Examples are: Colorado River Bridge, Topock, Ariz. (1890), span 990 ft; Little Colorado River "Cameron" Bridge, United States Highway No. 69 (1912), span 660 ft; Carquinez Straits Bridge, California (1927), span 1,100 ft; Cooper River Bridge, Charleston, S. C. (1929), span 1,050 ft; and Columbia River Bridge, Longview, Ore. (1930), span 1,200 ft.

(*b*) Studies leading to the construction of bridges with triangular cross section (like the double-track railroad Ruhr River bridge at Düren, Germany (1928)), and to the construction of three-plane plate girder bridges (like those in Hamburg, Germany (1928)), had the effect of substituting investigations based on scientific facts in place of rule-of-thumb width-length ratios. These developments were followed by framing bridges of rectangular cross section for space action (since 1918 applied in reinforceing existing bridges) and by designing middle support bridges in an inverted T-arrangement. In the latter types a single truss or arch rib or cable is combined with the floor to produce an efficient bridge structure. The purposeful utilization of the torsional characteristics of various arrangements stripped the concept of the width-length ratio of structural meaning. The spacing of trusses cannot be justified, therefore, by a desirable width-length ratio, but only by investigations of the torsional rigidity and the dynamic stability of the bridge.

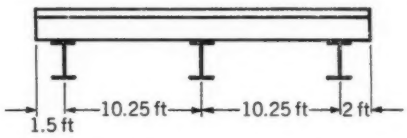
Floor System.—The floor steel in the Dubuque truss span (curbs, cross-beams, stringers, and floor beams) weighs 850 tons, which is equal to the metal content of the trusses and the floor system of the Davenport suspension bridge. The authors state (under the heading, "Design Loads; Live Load"): "* * * The floor system was designed for an H-20 loading on 9-ft lanes in accordance with the 1935 AASHO [American Association of State Highway Officials] specifications." They state further (under the heading, "Design Procedure: Floor System") that:

"Crossbeams, designed as continuous beams over the stringer supports, are 12-in. I at 31.8 lb in intermediate panels and 12-in. I at 40.8 lb in end panels. Stringers were designed as simple beams spanning between floor beams with maximum load reactions transmitted by the crossbeams, * * *."

The 1935 AASHO specifications require that the load on the stringers "* * * shall be the reaction of the truck wheels, assuming the flooring to act as a simple beam between stringers."

Influence lines of load distribution on the stringers are presented in Fig. 11. Evaluation of these curves for H-20 (1935) and H-20 (1941) loadings in Table 7 show that the AASHO specifications (curves 1, Fig. 11) give a load distribu-

TABLE 7.—Coefficients of Design Wheel Loads for Load Distribution on Stringers



Line	Specification	A	B		C	Total	
			....[a]	....[b]		....[a]	....[b]
1	AASHO; H-20 (1935).....	1.534	2.244	....	1.683	5.484	....
2	Bergendoff and Sorkin.....	1.295	2.652	2.725	1.439	5.386	5.459
3	AASHO; H-20 (1941).....	1.317	2.050	....	1.439	4.806	....
4	Balog; H-20 (1935).....	1.736	1.597	1.620	1.839	5.172	5.195
5	Balog; H-20 (1941).....	1.578	1.552	1.592	1.675	4.805	4.845
6	H-20 (1935)[c].....	1.629	1.655	1.680	1.719	5.003	5.028
7	H-20 (1941)[c].....	1.539	1.598	1.622	1.651	4.788	4.812

[a] Corresponds to specifications of the American Association of State Highway Officials (AASHO) for the position of loads. [b] Corresponds to the position of loads giving maximum influence value. [c] Calculated by the writer by the use of "A Distribution Procedure for the Analysis of Slabs Continuous Over Flexible Beams," by Nathan M. Newmark, *Bulletin No. 304*, Eng. Experiment Station, Univ. of Illinois, Urbana, Ill., 1938; and "Moments in I-Beam Bridges," by Nathan M. Newmark and C. P. Siess, *Bulletin No. 336*, Eng. Experiment Station, Univ. of Illinois, Urbana, Ill., 1942.

tion that is closer to the actual than are the continuous beam reactions, which the authors state were used in the design. The load distribution on the stringers and the stresses in the crossbeams are correlative. Consequently the authors' design stresses for the crossbeams also differ from the actual stresses. The stringers deflect under the loads; hence, the load distribution and the stress values used by the authors cannot occur. Table 7 indicates that 1941 AASHO specifications give the correct load on these three stringers, although they fail to give the correct distribution between them. Apart from the extensive foreign language literature on the solution of this floor problem, studies on this subject were published in the United States in 1938[21] and in June, 1942.[22]

Floor arrangements shown in Table 8 for the Dubuque truss span contain from 76 tons to 113 tons, and from 143 tons to 179 tons, less carbon steel or

[21] "A Distribution Procedure for the Analysis of Slabs Continuous Over Flexible Beams," by Nathan M. Newmark, *Bulletin No. 304*, Eng. Experiment Station, Univ. of Illinois, Urbana, Ill., 1938.

[22] "Moments in I-Beam Bridges," by Nathan M. Newmark and C. P. Siess, *Bulletin No. 336*, Eng. Experiment Station, Univ. of Illinois, Urbana, Ill., 1942.

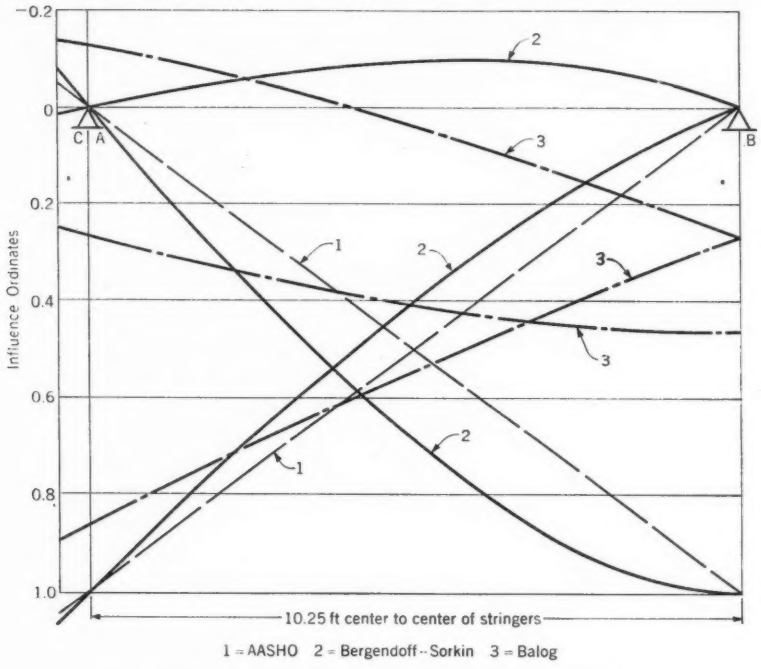


FIG. 11.—INFLUENCE LINES OF LOAD DISTRIBUTION ON STRINGERS

TABLE 8.—WEIGHT OF STRINGER METAL IN VARIOUS FLOOR DESIGNS OF THE DUBUQUE TRUSS SPAN (FLOOR BEAM SPACING, 38.42 FT)

Type	Design method	Carbon Steel[a]		Silicon Steel[a]		Carbon Steel		Silicon Steel	
		Total	Saving	Total	Saving	Section	Wt.[a]	Section	Wt.[a]
(*a*)	Authors	575	....	....	....	One, 33WF125 One, 36WF150 One, 30WF116 12 I 31.8 at 4.8 ft 12 I 40.8 at 4.8 ft	575	....	....
(*b*)	Balog	456	91	324[b] 79[c] 403	132	Three, 33WF125 One diaphragm (I = 2,681 in.⁴) Two, 14WF34	456	Three, 30WF108 One diaphragm (I = 1,967 in.⁴) Two, 14WF34	324[b] 79[c]
(*c*)[d]	AASHO (1935)	428	113	342	179	Four, 30WF108	432	Four, 27WF91	364
	AASHO (1941)	412	125	342	179				
	Balog	392	141	348	174				
(*d*)[d]	AASHO (1935)	476	76	388	143	Five, 27WF91	455	Five, 24WF74	370
	AASHO (1941)	455	92	370	157				
	Balog	435	107	370	157				

[a] Weights, in pounds per foot of bridge. [b] Silicon steel. [c] Carbon steel. [d] The weights of the cross frames at midpanel are omitted from types (*c*) and (*d*) to make the comparisons equitable with the authors' design, type (*a*).

silicon steel, respectively, than the bridge as built. In the layouts shown in Figs. 10(*b*) and 10(*c*), the floor beam spacings are one half and one quarter, respectively, of the panel length. These floors contain 380 tons less metal than the floor as built. In 1938 the writer made an extensive comparative investigation of the type of floor system used by the authors and a five stringer arrangement. The actual transverse and longitudinal load distribution was considered in both systems, and even then type (*a*), Table 8, cost more.

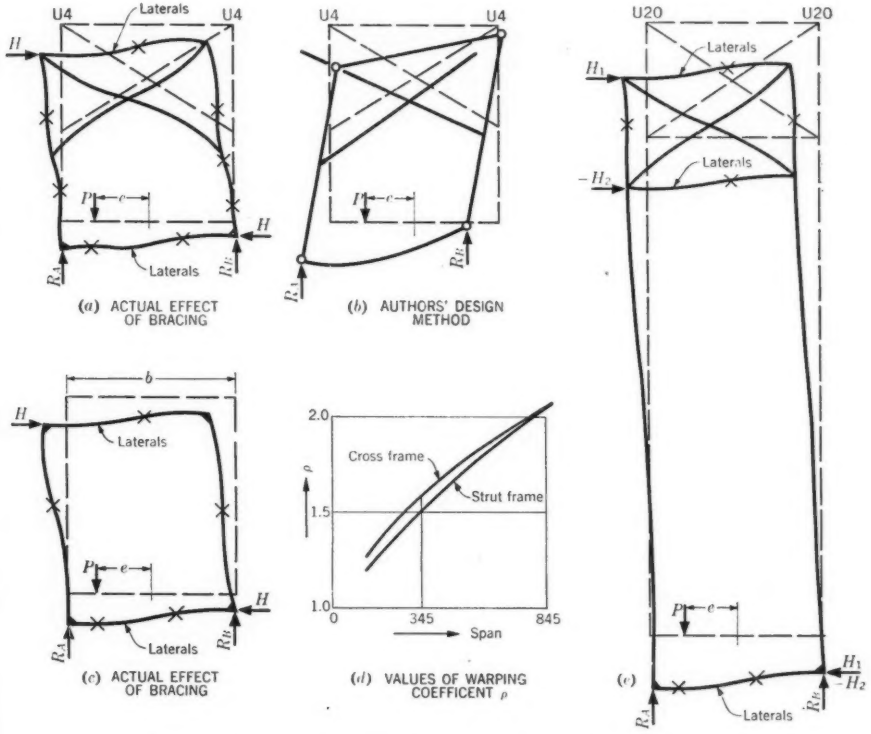


FIG. 12.—EFFECT OF BRACING ON THE DISTRIBUTION OF APPLIED LOAD

Lateral and Sway Bracing.—Bracings may be designed: (1) For carrying all types of loads acting on the bridge; or (2) for carrying loads acting in their planes only. In the latter case the bracing is assumed to be pin-connected to the main trusses and the truss sections are not reduced on account of bracing participation. Although the authors assumed this latter action, they provided rigid connections. The Dubuque truss span has three lateral systems—double-intersection sway frames at every second panel point, special sway frames, and double-intersection portals, containing 600 tons of steel. In comparison, the weight of bracing metal in the bridge (Fig. 10(*c*)) is 280 tons.

The stress condition assumed by the authors in distributing loads acting on and between the main trusses is possible only if the floor beams were simple

beams and the bracing were free from stress. Fig. 12(*b*) indicates the deformation of a transverse section consistent with these assumptions—near the point of application of the load the chord has the greater stress and elongation; consequently, it must move outward away from the load.[23] In 1930 and 1931, Rudolf K. Bernhard,[24] M. ASCE, reported 7,800 and 360 field measurements, respectively, on eight bridges, some framed like the Dubuque truss span, proving that: (*a*) Because of eccentric floor loads, transverse sections tilt in the direction opposite to that resulting from the authors' assumptions; and (*b*) the chord stresses tend toward equality with increasing transverse stiffness of the spans (Fig. 12 (*a*)). Furthermore, he showed that, in a truss span of triangular cross section, the stresses in the chords are always equal, irrespective of the eccentricity of the load. In other words, an eccentric load is equivalent to a centric load plus a moment. The centric load produces equal stresses in the trusses and the moment rotates the section formed by the main and the lateral trusses. When such a rectangular section is rotated, without any change in the diagonal distance between the chords, the chords (the corners

TABLE 9.—EFFECT OF THE BRACING ON THE DISTRIBUTION OF LOAD (S = BERGENDOFF AND SORKIN; B = Balog)

Section	Width *b*, in feet	LOAD $P = 1$ ON TRUSS A; FIG. 12						MOMENT 0.5 $P\,b$ CARRIED BY:[a]			
		Reaction R_A		Reaction R_B		Reaction H		Main Trusses		Laterals	
		S	B	S	B	S	B	S	B	S	B
Fig. 12(*a*).....	35	1	0.770	0	0.230	0	0.193	100	54	0	46
	29	1	0.700	0	0.300	0	0.209	100	40	0	60
Fig. 12(*c*).....	35	1	0.795	0	0.205	0	0.171	100	59	0	41
	29	1	0.734	0	0.266	0	0.185	100	47	0	53
Fig. 12(*e*).....	35	1	0.644	0	0.356	0	0.096[b]	100	29	0	71

[a] Percentage of total moment. [b] $H_1 - H_2 = 0.519 - 0.423 = 0.096$.

of the section) will remain free from stress and the sides of the section (web members and laterals) will be stressed. The sum of the stresses resulting from the centric load and the moment give the true stresses in the system. Therefore, the distribution of eccentric and lateral loads depends on the warping resistance of the span.

In Table 9 the load distributions in the Dubuque truss span, as determined by the use of empirical warping coefficients (B) and the authors' values (S), are compared. According to the writer's computation, the lateral systems carry 46% and 71% of the moment created by a load acting at one truss only, at panel points 4 and 20, respectively. Therefore, the effect of the bracing is greater in the more flexible, longer span. Table 9 also contains distribution values for a strut frame and for a width 6 ft smaller than the 35 ft used. In the narrower bridge, bracings of the same type effect a more even distribution of

[23] "Über den Einfluss der senkrecht wirkenden Verkehrsbelastung auf die waagerechten Verbände eiserner Balkenbrücken," by W. Bergfelder, Technische Hochschule, Aachen, 1928.

[24] "Über die Verwindungssteifigkeit von zweigleisigen Eisenbahnfachwerkbrücken," by Rudolf Bernhard, *Der Stahlbau*, Vol. 3, 1930, pp. 85–92, and Vol. 4, 1931, pp. 241–244.

an eccentric load; consequently, larger stresses are created in the bracing. The authors' method of design resulting in the same load distribution for any span length, width, and type of bracing yields results that differ considerably from those derived from measurements on actual bridges and shown by the writer in Table 9. In 1944, Louis E. Vandegrift reported measurements[25] on the three-span, continuous truss, Scioto River bridge east of Chillicothe, Ohio (1939), which further confirm the writer's opinion concerning the action of the bracing. This structure has a 24-ft roadway between the trusses and a 5-ft sidewalk outside the south truss. Both trusses were found to oscillate at practically the same frequency despite the additional sidewalk mass outside the south truss. Measurements[26] made on the Illinois River bridges at Hennepin and Lacon in Illinois (1940) indicated that the change in reactions resulting from warping the spans is more than ten times as much as that caused by raising or lowering the trusses by the same amount, proving also the effective framing action of the bracing.

Heinrich Müller-Breslau[27] demonstrated in 1904 that the stresses in a system composed of the main and lateral trusses of an arch differ greatly from the stresses obtained in the plain three-span truss assumed by the authors in this design. The tied arch truss bridge was the standard type of structure in Central Europe between 1880 and 1920 for both railway and highway bridges longer than 240 ft, and a wealth of experience was accumulated concerning its bracing requirements. The clear, clean, and efficient bracing systems of the double-track railroad bridges over the Rhine River in Germany at Rüdesheim (1913), Engers (1915), and Remagen (1916), with arched spans of 566 ft, 617 ft, and 512 ft, respectively, bear testimony to this fact. None of these bridges had lateral bracing at the lower arch chord, nor any conspicous sway bracing, despite the fact that they were designed for larger wind loads and for a centric and eccentric live load seven times that of the Dubuque truss span. A single strut type of portal was found to be sufficient for rigidity and stress requirements.

Analyses and model studies made for the 1,675-ft Kill van Kull (Bayonne) arch,[28] connecting Bayonne, N. J., with Port Richmond, Staten Island, N. Y. (1931), disclosed that the sway bracing between the lateral systems at the upper and lower arch chords at every panel point throughout the span reduced the downward deflection of the truss loaded at the quarter point to 60%. When the sways in the 1,240-ft through section of the span were omitted, the deflection of the loaded truss was reduced to 55%. Therefore, the transverse bracing only in the 206-ft end deck sections of this 1,675-ft span was found to be sufficient to distribute the stresses, and to equalize the deflections, between the two arch ribs in all cases of unsymmetrical loading throughout the span. The model (1:192) also ascertained the true action of the portals; thus all

[25] "Vibration Studies of Continuous Span Bridges," by Louis Edmond Vandegrift, *Bulletin No. 119*, Eng. Experiment Station, Ohio State Univ., Columbus, Ohio, July, 1944.

[26] "Adjustment of Continuous Bridge Affected by Warping," by L. T. Wyly, T. I. Fullenwider, and R. B. Murphy, *Engineering News-Record*, Vol. 125, 1940, p. 280.

[27] "Beiträge zur Theorie der Windverstrebungen," by Heinrich Müller-Breslau, *Zeitschrift für Bauwesen*, Vol. 54, 1904, p. 115, and Vol. 55, 1905, p. 133.

[28] "The Design, Materials and Erection of the Kill van Kull (Bayonne) Arch," by Leon S. Moisseiff, *Journal of the Franklin Institute*, Vol. 213, 1932, pp. 465–502.

unnecessary bracing was eliminated, and, consequently, the dead load was reduced, saving 600 tons of steel and improving the appearance of the bridge. This bridge was designed for a live load of 7,000 lb per ft, a horizontal transverse wind load of 2,000 lb per ft, and a temperature variation of ±55° F.

The stability conditions of the lower arch chord of the Bayonne arch and those of the Dubuque truss span are considerably different. In the former the lower arch chord carries practically all the dead load and is always highly stressed in compression; in the latter the crown section, 20% of the length of the span, is always in tension and, in the adjacent 20% lengths of the span, compression is low. These facts, in conjunction with the foregoing examples, both for the true action of the bracing and for the efficient bracing arrangements of actual large bridges, support the writer's view that the bracing of the Dubuque truss span is inconsistent with both, and with newer trends in bridge design. The broad, rigid, hangers seem not to be in keeping with the authors' emphasis on an unobstructed view to traffic, and they are inconsistent with the use of two lateral systems and sways for connecting the curved chords, because these rigid hangers carry bending and torsional stresses without benefiting the structure; wire ropes or high-strength round bars might well have been substituted.

Erection Procedure.—Under the heading, "Conclusions," the authors state:

> "A significant advantage of the continuous tied arch structure as compared to the conventional truss types is the economy of metal, due to a more advantageous distribution of the weight for cantilever erection."

Balanced against such economy, in the writer's opinion, is the fact that 345 tons of excess metal was required to meet the temporary requirements of erection. This weight constitutes 13.5% of the steel in the trusses and is equivalent to an increase in dead load to one third of the design live load. Erection considerations, being subject also to many unforseen circumstances, should not govern the design. The designer should rather concentrate his best efforts on the problem of meeting the requirements of the finished structure most economically, leaving the erector the responsibility for getting the bridge into place. If any change in the structure is required, that is subject to his approval.

Summary.—The writer believes that the river crossing at Dubuque could have been designed and erected by the use of considerably less metal than was actually used. The savings would be in floor steel, bracing members, excess steel used for erection, and increase in truss sections to support extra dead loads and wind loads that would have been avoided in other types of structures.

AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

DISCUSSIONS

SOME EFFECTS OF ANAEROBIC DIGESTION ON SEWAGE SLUDGE

Discussion

BY W. E. ABBOTT, AND ROBERT E. LEAVER

W. E. ABBOTT.[9]—The reduction in volatile solids through digestion for 30 days is of the order of 50% (Tables 5 and 6). Therefore, the proposed discharge of digested sludge will decrease the volatile suspended matter reduction resulting from primary sedimentation by one half—that is, if primary sedimentation removes 70% of the suspended matter, the effluent with the digested sludge will contain 100% minus 35%, or 65% of the initial volatile suspended matter instead of 30%. In other words, volatile suspended matter of the settled sewage is more than doubled through the addition of the digested sludge. The proposed addition of sludge in effect cuts the efficiency of the sedimentation tanks by rather more than half. Surely this is the truest measure of performance of the suggested sequence of operations.

The writer is reluctant to apply the 5-day B.O.D. (biochemical oxygen demand) test to sludges of any type; least of all to digested sludge with countless numbers of anaerobic organisms. The B.O.D. results in the paper appear to be a commentary on the futility of measuring dissolved oxygen changes in diluted sludges rather than evidence that the oxygen balance of a stream will benefit.

ROBERT E. LEAVER,[10] ASSOC. M. ASCE.—Several problems involving the disposal of sludge by the method advocated in this paper are of interest to the Washington State Department of Health and the Pollution Control Commission of the State of Washington. One example is at Portland, Ore., across the Columbia River from Washington. The aforementioned official agencies question the desirability of going to the expense of removing sludge and then, to accomplish an apparent saving, allow sludge, digested for varying lengths of time, to be returned to the effluent. Digested sludge properly utilized is a valuable natural resource and it seems inexcusable to waste it to the degradation of streams. It is becoming common practice to utilize sludge in the liquid

NOTE.—This paper by A M Rawn and E. J. Candel was published in November, 1948, *Proceedings*.

[9] Chemist, Nottingham Sewage Disposal Dept., Nottingham, England.

[10] Senior Public Health Engr., Washington State Dept. of Health, Seattle, Wash.

state, thereby reducing the need for sludge beds. In the State of Washington, the use of liquid sludge under pressure to fertilize beneath the surface of the ground in orchards, hop fields, etc., is gaining much favor. It is granted that considerable effort will be required in certain localities to develop a market for sludge. However, where this effort has been made the economic returns have justified it.

The paper by Messrs. Rawn and Candel opens many questions, one of which is the significance of the 2.1% increase in the 5-day (20° C) B.O.D. (biochemical oxygen demand) in the case of a stream or river below the treatment plant. The organic matter producing B.O.D. in the primary effluent, in general, will remain in suspension and will be further digested by natural aerobic conditions whereas, for the most part, the partly digested sludge will settle to the bottom of the stream and go through benthal digestion, which often requires years.[11] This sludge can accumulate to the extent that dissolved oxygen could be affected adversely. It would appear from this characteristic that the B.O.D. "picture" on sludge must be interpreted from an entirely different standpoint than is normally considered reasonable in connection with the 5-day (20° C) B.O.D.

As Messrs. Rawn and Candel state, their method of running B.O.D. analyses on sludge "is neither true nor reliable." When dealing with sludge, the high initial demand and lag combine to obscure the normal monomolecular reaction which characterizes a B.O.D. test.

Even in the rapidly moving rivers of the State of Washington, sludge beds can, and do, form where the settleable solids are not yet removed by treatment. The slowing down of velocity along the banks and bends will cause deposits which are most accessible and can cause the greatest nuisance if they are composed of sewage sludge. In fact, the writer does not know of any streams in the State of Washington where sludge beds will not form.

The effectiveness of the chlorination of digested sludge requires additional study since it is well known that solids give bacteria protection against chlorination and that chlorine residuals are not the complete answer.

The increase of settleable solids in the primary effluent by 1.5 ml per liter by the return of digested sludge does not appear too important to the casual reader, but as a result at least 50.4% of the total solids that were removed to the digester are again returned. Certainly this amount is not insignificant.

The practical difficulties in preventing some of the scum from being carried out with the digested sludge and the 21% to 31% theoretical grease content increase make the evaluation of expected conditions, in a particular case, difficult to say the least.

This paper certainly has stimulated considerable thought on the subject of anaerobic digestion and has shown sanitary engineers in the State of Washington that additional studies are desirable. The Civil Engineering Department of the University of Washington at Seattle has agreed to conduct certain research to try to answer some of the questions that have been advanced in this discussion.

[11] "The Natural Purification of River Muds and Pollutional Sediments," by Gordon M. Fair, Edward W. Moore, and Harold A. Thomas, Jr., *Sewage Works Journal*, March, 1941, p. 270, July, 1941, p. 756, and November, 1941, p. 1209.

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DISCUSSIONS

VALUATION AND DEPRECIATION OF PUBLIC UTILITY PROPERTY

Discussion

BY MAURICE R. SCHARFF

MAURICE R. SCHARFF,[51] M. ASCE.—Messrs. Kappeyne, Gunn, Lippitt, Marston, and Learned all emphasize the difficulties and uncertainties involved in estimates of value of service, and corresponding value of property and depreciation, such as the writer has suggested. In addition, Dean Marston points to the contribution to such uncertainty of "* * * the conflicting testimony of experts, paid by opposing parties in court proceedings."

The writer fully agrees that there are difficulties and uncertainties involved in the procedure which he suggested. He believes, however, that three comments should be made regarding the quoted part of the discussions.

In the first place, if "Each such party can be sure that it can hire a battery of 'experts' to support its particular contentions" the professions of engineering and economics have failed to secure acceptance of professional standards of conduct by "experts" from their ranks; or to secure recognition by industry and finance of the value of genuinely professional opinion as distinguished from readiness to support particular contentions. In either case it can never be hoped that any method of proof of value within the framework of the American economic system can ever be established, and the system itself will eventually be impaired by the persistence of "experts," industrialists, and financiers in these practices. Discussions of methods of valuation are worthwhile only in terms of professional competence and conduct, and belief in the ability of a democratic capitalistic society to work out and accept sound conclusions.

In the second place, difficulty and uncertainty in establishing a sound conclusion are not sufficient grounds for its rejection and for acceptance of a conclusion that is unsound. Mr. Lippitt has made a helpful contribution in calling attention to the Minority Report of the Commission on the Revision of the Public Service Commission Law in New York on these difficulties and uncer-

NOTE.—This paper by Maurice R. Scharff was published in June, 1948, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: October, 1948, by J. Kappeyne; November, 1948, by W. V. Burnell; December, 1948, by Donald Gunn, Henry F. Lippitt, II, and Albert P. Learned; and February, 1949, by Anson Marston, and James P. McKean.

[51] Cons. Engr., New York, N. Y.

tainties, and to certain legal opinions which he cites as bars to acceptance of the writer's proposal. Nevertheless, it has been the writer's attempt to suggest that the regulatory commissions and courts have fled from these uncertainties and difficulties to unsound procedures of valuation and rate making, and the emphasis on the obstacles referred to is not a conclusive answer to the writer's contention that his proposal accords with sound engineering economic concepts.

In the third place, it is the writer's belief that the difficulties and uncertainties referred to, great as they are, and great as they have always been in connection with all valuation procedures, are, nevertheless, exaggerated. It has been the experience of the writer that substantial agreement between qualified engineers, who adhere to professional standards, is not too difficult to reach on what constitutes modern efficient design and the cost factors that correspond with it. The real requirement is for clarification of the principles under which these engineering-economic factors are to be applied.

Messrs. Kappeyne, Gunn, and Lippitt also indicate that the writer has not succeeded in clarifying the distinction he sought to emphasize—between valuation and rate making on the one hand, and original cost and accounting on the other. It was intended to suggest that it would be advantageous if complete separation could be restored between these two groups of concepts. The only suggestion as to any continued relation between them was for the use of proportionate loss in value as a measure of depreciation reserve requirement in accounting (as a substitute for the more conventional straight-line or compound interest reserve requirement standard) in the undesirable case that the regulatory commissions continue to insist that depreciation reserves represent "approximately the amount of depreciation actually existing in the property."

Mr. Kappeyne argues that investors who furnish the capital required for a utility:

> "* * * not only are entitled to the protection of their investment, but also are entitled to the opportunity of earning a return thereon that they had good reason to expect at the time the investment was made."

Mr. McKean quotes from the very interesting article of Everett C. McKeage[43] to somewhat the same effect. These arguments (which are commonly advanced in support of the prudent investment or original cost less accrued depreciation theory of rate making) seem to the writer to be based on the erroneous assumption that the property right in public utility property is substantially different from that in other private property.

In a dissenting opinion, the late Mr. Justice Louis D. Brandeis, joined by the late Mr. Justice Harlan F. Stone, in New State Ice Company versus Liebmann (285 US 262) stated that:

> "The notion of a distinct category of business, 'affected with a public interest,' employing property 'devoted to a public use' rests upon historical error. * * * In my opinion, the true principle is that the state's power extends to every regulation of the business reasonably acquired and appropriate for the public protection."

[43] "Public Utility Property: Views of Commission Counsel As to Valuation," *Journal*, Am. Bar Assn., December, 1948, p. 1156.

This statement seems to the writer to be the sounder doctrine. The real question, therefore, is whether the scope and the character of the regulation applied are reasonably required and appropriate for the public protection; and it is the writer's contention that this criterion is met within the framework of a competitive capitalist system only when the concept of the value of the service, as measured by competitive cost, is applied.

Furthermore, the writer is confident that those who rely on any assumed guarantee by the public of the right to recover their investments, however prudent at the time they were made, or to continue to earn a return on such investments less some accounting reserve for depreciation, after some major technological or economic change which substantially reduces the competitive cost of service or limits the market for sales because of the competition of alternative services, are deluding themselves. A substantial decrease in the cost of generating electric energy, for example, such as might conceivably result from developments in the field of atomic power, would inevitably be followed, in the opinion of the writer, by a reduction in the earning power and value of investments in steam boiler plants—and, if the scientists succeed in converting nuclear energy to electric energy without the intervention of the heat cycle, the effect might readily extend to turbo generator plants as well. The effect of the competition of alternative services on the earning capacity and value of street railway properties, and to a lesser extent on the earning capacity and value of manufactured gas properties, is also too well known to require further comment.

The same error, as well as failure to appreciate the distinction (which the writer attempted to make between valuation and accounting, as previously referred to), is involved in Mr. Lippitt's discussion of the correlation of annual and accrued depreciation. If, as the writer contends, rate making is a species of economic price fixing unrelated to accounting, and if accounting is a conventional method of amortizing investments unrelated to valuation and rates, the entire concept that annual and accrued depreciation are "contributed by consumers" is meaningless.

Mr. Burnell, although expressing general agreement with the paper, raises one question which the writer is not sure he fully understands. To exemplify his question, Mr. Burnell selects the item of coke ovens, with a service value of $1,165,800, and a future capitalization period of 25 years. Then he states:

> "The next implication of the process is that the accumulated amount of the depreciation reserve for the present substitute plant at the end of the 25-year period, if today's assumptions of remaining life are consistent, must also be $1,165,800."

The writer does not understand why such a relation is implied. In his opinion, the only implication is that the annual depreciation on the $1,165,800, compounded for the future capitalization period of 25 years, must equal the same amount. The facts as to this relation are, that the sinking fund payment factor for 25 years is 0.0182267, which, applied to the service value of $1,165,800, gives an annual sum of $21,248.70. The present-worth factor for an annuity

of one per year for 25 years is 54.8645 and when this is multiplied by $21,248.70 it gives $1,165,800.

Mr. Burnell also suggests that public utility property can be kept in physical condition for service indefinitely by adequate maintenance and replacement of parts, and that the only difference between the value of a new property and an old one is the capitalized value of the difference in their annual cost of maintenance and physical upkeep; and Mr. Learned suggests that the writer's own proposal, in the absence of changes in the arts, would correspond with no increase of accrued depreciation unless unit operating and maintenance costs increased with age. These contentions seem to leave out of account the facts that every estimate of value is implicitly, if not explicitly, an estimate of the present worth of an annual advantage from the use of a capital good for a future capitalization period; and that the future capitalization period of an old capital good which enters into such implicit or explicit present-worth estimate is less than the future capitalization period of a new capital good.

Mr. Gunn suggests that he would never recommend a capital expenditure on the basis of a capitalization period, ranging up as far as 80 years, and that for such purposes periods of more than 20 or 30 years would be unrealistic. The writer agrees with him that the evaluation of depreciation during the useful life of property must be governed by the same principles that justified its instalment and by which its replacement or abandonment will be dictated; but he ventures to doubt that Mr. Gunn really intends to suggest that the justification of the capital expenditures for the dams, involved in the cases to which he refers in his discussion, should have been based on capitalization periods of no more than 20 or 30 years.

Mr. Gunn also suggests the separate treatment of depreciation due to physical causes and that due to economic causes. This is a point of view to which the writer adhered in 1939 when he contributed his paper on "Depreciation and Obsolescence."[52] He has subsequently come to recognize more clearly that value of service, and value and depreciation of property, are indivisible economic concepts, and that the effort to measure, separately, depreciation due to physical deterioration (or to take it into account, in connection with obsolescence, by "observation," as Mr. Learned suggests) is futile.

Dean Marston suggests that the correct application of the essential principle of Smyth versus Ames is that values are determined by judgments and that:

> "Every factor which should affect such judgments must be considered and these factors given their 'just and right' weights in each case—as determined by sound judgment, not by any formula."

He then goes on to suggest that the present trend is

> "* * * to give more and more weight to fully itemized property inventories showing the actual original costs of the individual items of the actually existing properties; and to only such overhead percentages of cost as have been actually incurred in the past; but not excluding such substitution of present prices (and the resulting cost values of individual items)

[52] *Transactions*, ASCE, Vol. 104, 1939, pp. 1127–1138.

as sound, conservative judgment deems certain to prevail for a considerable future period."

This application of the Smyth versus Ames principle makes the determination of value entirely subjective, and it has been the writer's intention to suggest that it has led to the present confusion. Indeed, he would go further and argue that it was the failure of the engineering and economic professions to develop a sounder application of the Smyth versus Ames decision that led to the extreme development of the subjective approach which reached its climax in the case in Wisconsin of the City of Two Rivers versus the Commonwealth Telephone Company.[53] In that case the Wisconsin Public Service Commission carried the subjective approach to its logical conclusion and stated:

> "* * * after careful consideration, we have concluded that reasonable rates are not properly arrived at merely by means of a formula whereby a percentage figure (which is deemed 'a reasonable rate of return') is applied to another figure which is termed a rate base. * * *"

and they stated further:

> "The rates herein prescribed are estimated and intended to afford approximately an annual net profit of a determined number of dollars which we think it reasonable for the utility to enjoy from the operation of its business."

This determination was reversed by the Wisconsin Circuit Court of Dane County[54] in deciding the case of the Commonwealth Telephone Company versus the Public Service Commission and the reversal was affirmed by the Supreme Court of Wisconsin[55] on the ground that its inherent fallacy appeared in the statement just quoted, and that there was no way for either the Commission or a reviewing court, or a utility, or the public, to determine whether the profit which the Commission thought proper was actually proper unless the Commission made specific findings of the "relevant facts and circumstances." "It was not the intention of the legislature," the Supreme Court of Wisconsin stated, "to bestow such arbitrary powers upon the Commission, and nothing in the statutes can be so construed."

The writer called attention, at the conclusion of his paper, to a decision of the United States District Court for the Eastern District of New York in a condemnation case, holding that a procedure substantially identical with that proposed in the paper "would seem to provide a fair standard of value for present purposes" (65 Federal Supplement 333 and 71 Federal Supplement 248). Consequently, he feels that he should report the subsequent development to date in that proceeding (United States of America Versus 25.4 Acres of Land and 53¼ Acres of Land). The decision of the District Court was appealed to the United States Court of Appeals for the Second Circuit and in May, 1948, the Circuit Court handed down its decision, reversing the District Court and remanding the case for further hearing (168 Federal 2d; 391). The principal grounds for reversal were: First, that, in its acceptance of the "estimates of the

[53] *Public Utilities Reports*, New Series, Vol. 70, 1947, p. 5.
[54] *Public Utilities Reports*, New Series, Vol. 71, 1948, p. 65.
[55] *Ibid.*, Vol. 73, 1948, p. 97.

defendants' expert, accompanied as they were by an accumulation of imprecise theory in what is considered the strong tradition of American judicial valuation," the District Court "got itself into the position of assuming a loss before one was proven and of measuring it by the extent of curtailment of the facilities"; and, second, that the trial court declined to consider evidence as to increased sales of electricity and gas to the government after the date of the condemnation. The Circuit Court concluded that:

> "* * * the value at the time of taking must be developed, in default of a sale price, largely from a consideration of past earnings and such showing of prospective earnings as can be made."

Subsequently, in February, 1949, a third hearing was held in the United States District Court and the case was resubmitted for decision in accordance with the opinion of the Circuit Court. On April 6, 1949, just as this closing discussion was in course of preparation, the District Court handed down its opinion, holding as follows:

> "Since the evidence * * * is thought to supply that which was deemed necessary to completion of a proper record, there remains but to say that, while the figures previously adopted could reasonably be increased in light of the testimony taken and exhibits filed at the third hearing, to yield to the impulse so awakened might lead to misunderstanding; accordingly the decision will be that the court hereby makes and states the following awards in this proceeding, to these claimants. * * *"

after which the identical awards made after the second hearing before the District Court are restated.

In the course of this opinion, the District Court stated:

> "At the second hearing, the expert Scharff whose qualifications were not impaired by anything shown in the record, and who impressed me as a technically informed person, expressed his opinion as to a fair measure of compensation to be paid for the taking of these properties and franchises. His testimony is somewhat discussed in the opinion which appears in 71 Fed. Supp. 248 at 252 et seq., and consequently need not be further expounded. It has not been overlooked that his estimates have been characterized as being accompanied by 'an accumulation of imprecise theory.'
>
> "That statement may not be ignored by this court, but the Government has not developed it for present purposes, i.e., the imprecise theories have not been identified or discussed in any analytical sense.
>
> "Nor have contrasting theories been advanced by any witness except Mr. Shlichta, who thought that the useful life of these properties in their dual aspect was from 15 to 20 years, instead of the 40 years attributed to them by Mr. Scharff. Since the court used a 20-year period because that measures what is usually called a generation, it seems, with all deference, that Scharff's opinion has indeed not been discredited. Since it was an opinion and could be nothing more, and since it was offered by one whose training and experience invite respectful attention, I am aware of no reason why it should not be consulted by this court in reaching a conclusion."

At the close of the third hearing in the District Court, counsel for the United States stated that he would appeal again if an award were made, so that this case may yet become something of a "cause célèbre."